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THE

AMERICAN

JOURNAL OF PSYCHOLOGY

EDITED BY

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EDMUND CLARK SANFORD
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VOL. XXVI

ALBANY, N. Y. AND WORCESTER, MASS.
FLORENCE CHANDLER, Publisher
1915

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Psychology. Periodicals

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THE AMERICAN
JOURNAL OF PSYCHOLOGY
Founded by G. Stanley Hall in 1887

Vol. XXVI JANUARY 1915 No. 1

THE SENSATIONS OF THE ALIMENTARY CANAL[1]

By Edwin Garrigues Boring

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Historical

The problem of the sensibility of the internal organs has occupied the attention of more than one physiologist. Weber[2] in 1846 performed some simple experiments, which were, however, not conclusive, because his technique was inadequate. Recent work dates from the first publication of Lennander[3] in 1901. Lennander, chiefly on the basis of clinical data, put forward the hypothesis that the internal organs, including the stomach, intestine, liver, gall-bladder, kidneys, and the intervening tissues, are entirely insensitive. Sensations that seem to occur in these organs, such as the feeling of digestion or the pain from irritation, are mediated by the parietal peritoneum. Meumann[4] opposed the theory of Lennander in an article which re-

[2] Weber, E. H., Der Tastsinn und das Gemeingefühl, Wagner's *Handwörterbuch der Physiologie,* 1846, III, 562ff.

[3] Lennander, K. G., Ueber die Sensibilität der Bauchhöhle und über lokale und allgemeine Anästhesie bei Bruch- und Bauchopererationen, *Centralbl. f. Chir.,* 28, 1901.

[4] Meumann, E., Zur Frage der Sensibilität der inneren Organe, *Arch. f. d. ges. Psychol.,* 9, 1907, 26ff.

views both Lennander's work and subsequent publications. He calls attention to the prominence of organic sensations in daily life,—in digestion, in intestinal pressure, in hunger, in the pains of flatulence, in esophageal pains, in sensations from the lungs and from the heart, in micturition, in defecation, in colic, and in internal irritation. Becher[5] later published a partially experimental study, in which he described the sensitivity of the esophagus and concluded that the stomach is insensitive. In this article he leans toward Lennander's hypothesis, in that he ascribes the feeling of vitality and other complex feelings of an organic nature to sensations from the body-wall, the pleura, the peritoneum, the diaphragm, and, probably, the esophagus. He thinks that Meumann's sensations of the stomach must have come from the body-wall. He denies sensibility to the lung tissue, the heart, and the arteries. The veins mediate pain. Meumann replies[6] that the reference to the body-wall will not explain the difference in quality of the different stomachic sensations, or the difference of all of them from pressure; that the extension of the wall is too slight to give intense sensations; that the feeling of satisfaction is obviously dependent upon the chemical state of the stomach; and that experiments of his own with spices have demonstrated the sensibility of the stomach to one sort of stimulation. He also insists on sensations from the lung tissue and the heart, and cites experimental work upon animals to show that the large intestine, stomach, mesentery, peritoneum, spleen, and blood-vessels are sensitive to pain. While the controversy between Meumann and Becher continued, Hertz, Cook, and Schlesinger[7] published in England an experimental study, which showed, among other things, the unquestionable sensibility of the esophagus to thermal and mechanical stimulation. These results agree further with those of Head, Rivers, and Sherren,[8] who cite the colon as an example of 'protopathic' sensibility. Becher repeated[9] some of the experiments of the English workers, and convinced himself of the sensibility of stomach and intestine. He thus, eventually, came into practical accord with Meumann. At the same period, Ritter[10] and Kast and Meltzer[11] explained the apparent insensibility of the internal organs, reported by Lennander, by observations and experiments which went to show that normally sensitive organs are rendered insensitive by the subcutaneous injections of cocain used in local anesthesia. Lennander's observations were based upon opera-

[5] Becher, E., Ueber die Sensibilität der inneren Organe, *Zeitschrift f. Psychol.*, 49, 1908, 341ff.

[6] Weiteres zur Frage der Sensibilität der inneren Organe und der Bedeutung der Organempfindungen, *Arch. f. d. ges. Psychol.*, 14, 1909, 279ff.

[7] Hertz, A. F., Cook, F., and Schlesinger, E. G., The Sensibility of the Stomach and Intestines in Man, *Jour. Physiol.*, 37, 1908, 481ff.

[8] Head, H., Rivers, W. H. R., and Sherren, J., The Afferent Nervous System from a New Aspect, *Brain*, 28, 1905, 99ff.

[9] Einige Bemerkungen über die Sensibilität der inneren Organe, *Arch. f. d. ges. Psychol.*, 15, 1909, 356ff.

[10] Ritter, C., Zur Frage der Sensibilität der Bauchorgane, *Centralbl. f. Chir.*, 35, 1908, 609ff.

[11] Kast, L. and Meltzer, S. J., On the Sensibility of the Abdominal Organs and the Influence of the Injections of Cocain upon It, *Med. Rec.*, 70, 1906, 1017ff; Die Sensibilität der Bauchorgane, *Mitteilung. a. d. Grenzgeb. d. Med. u. Chir.*, 19, 1909, 586ff.

tions in which a local anesthetic had been used. On the ground of these papers, Meumann made a final plea[12] for the sensitivity of most of the internal organs; and later Hertz summed up, in a book,[13] the work of the English investigators in evidence of widespread sensibility. In the same year, however, Mitchell[14] published an account of cases, by means of which he endeavored to show that, to a great extent, abdominal pain originates in the parietal peritoneum.

Besides all this work upon the general sensitivity of the viscera, a great deal has been written upon the problem of hunger and appetite. Notable publications are those of Sternberg[15] and Turró.[16] Cannon[17] and Cannon and Washburn[18] have shown the dependence of hunger upon the contractions of the stomach, and Carlson[19] has since confirmed their results. These investigators have also identified hunger with a form of visceral pain.

All the work upon the internal sensibility preceding 1910 has been reviewed by Neumann,[20] who gives a bibliography of sixty-nine titles.

The problem of the sensibility of the inner organs is threefold,—physiological, psychophysical, and psychological. The physiological problem is that of the sensibility of the various organs. In what organs do the different sensations originate? Or, more specifically: where are the nervous terminations, involved in the impulses that condition the sensations, situated? Interest in the quality of sensation is secondary to interest in its conditions. The psychophysical problem is that of the correlation of sensation and stimulus. It is answered by a determination of limens, and of the adequacy of certain stimuli for certain qualities of sensation. It leads further to the question of the localization of the sensations in various parts of the

[12] Weiteres zur Frage der Sensibilität der inneren Organe, und der Bedeutung der Organempfindungen, *Arch. f. d. ges. Psychol.*, 16, 1909, 228ff.

[13] Hertz, A. F., The Sensibility of the Alimentary Canal, 1911.

[14] Mitchell, J. F., Sensibility of the Peritoneum and Abdominal Viscera, *Jour. Am. Med. Asso.*, 57, 1911, 709ff.

[15] Sternberg, W., Der Hunger, *Zentralbl. f. Physiol.*, 23, 1909, 105ff; Der Appetit in der experimentellen Physiologie und in der klinischen Pathologie, *ibid.*, 23, 1909, 305ff; Physiologische Psychologie des Appetites, *Zeitschr. f. Sinnesphysiol.*, 44, 1910, 254ff; Die Physiologische Grundlage des Hungersgefühls, *ibid.*, 45, 1910, 71ff; Der Appetit in der exakten Medizin, *ibid.*, 45, 1911, 91ff; Das Appetitproblem in der Physiologie und in der Psychologie, *Zeitschr. f. Psychol.*, 59, 1911, 91ff.

[16] Turró, R., Die physiologische Psychologie des Hungers, *Zeitschr. f. Sinnesphysiol.*, 44, 1910, 330ff; 45, 1911, 217ff, 327ff; Psychophysiologie de la faim, *Jour. de psychol.*, 7, 1910, 289ff, 409ff; 8, 1911, 332ff, 417ff.

[17] Cannon, W. B., A Consideration of the Nature of Hunger, *Pop. Sci. Mo.*, 81, 1912, 291ff.

[18] Cannon, W. B., and Washburn, A. L., An Explanation of Hunger, *Am. Jour. Physiol.*, 29, 1912, 441ff.

[19] Carlson, A. J., Contributions to the Physiology of the Stomach, *ibid.*, 31, 1913, 151ff, 175ff, 212ff, 318ff; 32, 1913, 245ff, 369ff; especially, The Relation between the Contractions of the Empty Stomach and the Sensation of Hunger, 31, 1913, 175ff.

[20] Neumann, A., Ueber die Sensibilität der inneren Organe, *Centralbl. f. d. Grenzgeb. d. Med. u. Chir.*, 13, 1910, 401ff, 449ff, 481ff, 529ff, 573ff, 617ff, 656ff, 696ff.

body. Interest is directed equally upon quality of sensation and upon the physical values of the stimulus or the anatomical position of the parts of the body. The psychological problem is that of the quality of the sensations. It requires an exact description of a certain class of experiences as they occur. Interest in the nature of the experience, and especially in its quality, is primary.

In the actual case, no one of these problems is entirely distinct from both the others. It may be said, however, that, so far, the physiological problem has been the most vigorously attacked. Weber, Lennander, Ritter, Kast and Meltzer, all the clinical workers, and all those who experiment upon animals ask first: What organs are sensitive? Meumann and Becher are but very slightly more psychological in their point of view, although they touch at times both upon the psychophysical problem of sensitivity and upon the qualitative nature of sensation. Hertz, Cook, and Schlesinger have much the same outlook. Meumann and Becher both speak of the ability to localize the sensations; they agree that with practice the organic sensations are easily localized, and that with this practice in localization their indefinite quality disappears. They do not, however, present definite data upon the form and amount of errors of localization.

The present study attempts to deal, from the psychophysical and psychological points of view, with the problem of the sensations arising from the stimulation of the alimentary canal. The writer has endeavored, by keeping a careful record of the intensities used, to note the dependence of sensation upon intensity of stimulus. He has attended to the direction, amount, and character of the errors of localization,—recording the localization, whenever it was mentioned, and performing a special experiment for the determination of the amount of reference for one class of stimuli. He has sought to obtain a description of the psychological character of the experiences, by the taking of full introspections upon all occasions. A statement of methods and results follows.

Experimental Work

I. *Procedure and Apparatus*

Observers and Experimenters. The following persons took part in the experiments:

B, the writer of this article, instructor in psychology; C, Miss J. N. Curtis, scholar in psychology; D, Mr. F. L. Dimmick, undergraduate, major subject in psychology; F, Dr. W. S. Foster, instructor in psychology; G, Miss M. E. Goudge, fellow in psychology; Ga, Mr. F. J. Gates, undergraduate, major subject in psychology; R, Mr. G. J. Rich, undergraduate, major subject in psychology.

The principal observer was B. He easily accustomed himself to the swallowing of the stimulus tubes, and his personal interest in

the experiment tended to offset the discomfort. He kept ahead of the other observers; all new series were first tried with him; and very many more were completed by him than by the others. C, D, Ga, R, and occasionally G acted as experimenters for him.

D, F, and G also acted as observers in all the work upon the esophagus and stomach. They completed the principal series with the various sorts of stimuli, although the data obtained from them were less extensive than those obtained from B. D readily learned to swallow the tubes, and in spite of a constitutionally weak stomach proved to be a very satisfactory observer. F also had little difficulty in learning to swallow the tubes, but was unable to retain them for long periods. As he never acquired the ability to talk with the tube in place, the tube had always to be removed before he gave a report. He was especially sensitive to the mechanical stimulation of the stomach and of the upper esophagus, and when the tube was in these positions frequently vomited before the stimulus could be given. G, who had a small pharynx, always swallowed the tube with great difficulty. When it was once in place, however, she found comparatively little difficulty in retaining it or in talking. R, after much patient effort, proved unable to overcome the gagging reflex and to get the tube past the pharynx, and could not be employed.

Stimulus Tubes. The following forms of tube were used for introduction into the esophagus or the rectum. They will be referred to hereinafter by number.

1. For thermal stimulation of the esophagus and for practice. Single tube, heavy wall, red rubber. Lumen, 3.5 mm.; outside diam., 7.0 mm. Used only in preliminary experiments and for practice in swallowing, on account of thinness of wall. A catheter was also used for practice.

2. For thermal and chemical stimulation of esophagus and stomach by injection of water or of chemical solutions. Double-walled[21] tube, made of ⅛ in. and 3/16 in. red rubber tubing. Lumen, 3.0 mm.; outside diam., 9.0 mm.

3. For thermal stimulation of esophagus by means of electric heating coil. Very smooth and stiff stethoscope tubing. Lumen, 5.0 mm.; outside diam., 9.0 mm. About the end of the tube is wrapped a coil of insulated copper wire. A hard rubber core is inserted within the tube at this place in order to render the coil firm. The coil consists of 3.8 meters of No. 30 double cotton-covered wire, resistance 1.5 ohms, and is 11 mm. long by 11 mm. in diam. The leads are of No. 22 double cotton-covered wire, and pass up through the lumen of the tube to heavier wires outside. The coil was soaked in rubber cement. All measurements along the tube are made from the center of the coil.

4. For thermal stimulation of the esophagus and stomach by contact with a metallic coil which conducts a current of water. Apparatus consists of a heavy outer tube of smooth red rubber, containing two small rubber tubes within it. Lumen of outer tube, 6.0 mm.; outside diam., 10.0 mm. Lumen of inner tubes, 3.0 mm.; outside diam., 5.0 mm. The inner tubes are thus forced into elliptical section within the outer tube. At the end of the tubes is a coil of brass

[21] The double-walled tube is necessary to prevent conduction. *Cf.* Becher, *Zeitschrift*, 49, 348; Hertz, *op. cit.*, 5.

tubing, 10 mm. long and 10 mm. in diam. The brass tubing has a lumen of 1.5 mm. and an outside diam. of 2.5 mm. It is coiled in 3¾ turns. The lower end of the coil is brought up through the center, and the two small rubber tubes are tied to the two ends of the coiled brass tube. The connections were soaked in rubber cement and drawn up within the outer tube, so that the coil was flush with the outside of the outer tube.

5. For mechanical stimulation of the esophagus by inflation of bladder.[22] Tubing as in 1. It bears on the end a rubber bladder (condum) about 5 cm. long, which inflates to 5 by 4, 6 by 5 cm., *etc.* The bladder is attached to the tube by rubber cement and by wrapping with heavy thread. At the point where the bladder is wrapped, a short piece of hard rubber tubing is placed inside the soft tube, so that the wrapping can be made firm. The joint is about 1.0 cm. long. It is soaked in rubber cement. All measurements along the tube are made from the middle of the bladder.

6. For mechanical stimulation of the stomach by inflation of bladder. Exactly as 5, except that the bladder is about 15 cm. long. Measurements along the tube are made from the proximal end of the bladder.

7. For bipolar electrical stimulation of esophagus and stomach by electric shock. Stethoscope tubing as in 3. Bears at the end two tin-foil rings, each 12 mm. broad. The lower ring is 10 mm. from the end; there are 8 mm. between the rings. Two lengths of No. 24 double cotton-covered wire are carried through the lumen of the tube, and the bared ends are brought out at the positions of each ring and wrapped about the tube over a hard rubber core within. The strips of foil which constitute the rings are wrapped over the wires and held by rubber cement. (This arrangement was less satisfactory than that used later in 8, because the contacts were slightly rough and scratched the throats of the observers.) Measurements were made from a point midway between the two contacts.

8. For electrical stimulation of various points of the esophagus and stomach, without shift of the position of the tube. Tubing as in 3. It bears six pairs of contacts which are, on the average, 0, 15, 20, 25, 30, and 35 cm. respectively from the lower end of the tube (*i. e.*, if measured to a point midway between each pair.) The contacts consist of brass rings, each of which is 6 mm. wide; inside diameter, 8.5 mm.; outside diam., 9.5 mm. Since the tube is 9 mm. in diameter, the rings indent it slightly and thus afford a fairly smooth surface. The contact at the lower end is a rounded cap, which fits over the end of the tube; it is held in place by a rivet. From each one of the twelve contacts a wire is led through the lumen of the tube. The wires are No. 22, copper, silk and cotton triple-covered. (A heavy insulation is necessary to prevent the induction shocks from jumping.) They are connected at the upper end of the tube to heavier wires leading to mercury cups. At each contact the wire pierces the wall of the tube and is soldered to the contact. Each connection is filed smooth, and the tube is filled at the point with rubber cement in order to prevent short-circuiting of the wires by water or by the acid of the stomach.

9. For thermal stimulation of the rectum by injection of water. Double-walled tube, consisting of a heavy rectal tube through which a lighter tube has been run. Lumen, 3.5 mm.; outside diam., 11.0 mm.

[22] *Cf.* Becher, *Zeitschrift*, 49, 350; Hertz, *op. cit.*, 16.

10. For chemical stimulation of the rectum by injection of liquids. Plain, single tube of same tubing as 3.

11. For mechanical stimulation of rectum by inflation of bladder. Of same tubing as 3. Bears on the end of it a bladder, 7 by 4 cm., attached in the same manner as that of 3.

12. For bipolar electrical stimulation of rectum by induction shock. Same as 7, except that the tin-foil is less smooth and is tied on with thread.

Arrangement of Apparatus. A description of the apparatus employed with the tubes for the control of the various stimuli —thermal, mechanical, electrical, chemical—follows.

For the introduction of hot and cold water into the alimentary tract the heavy-walled tubes 2 and 9 were employed, in order to retard conduction by the walls of the tube and to prevent consequent direct stimulation of throat or anus. Still further to reduce the possibility of conduction through the walls, an apparatus (Fig. 1) was devised by means of which a given amount of water at a given temperature could be forced through the tube so rapidly that there was no time for the tube to cool down or to warm up materially. A 100 cc. flask is placed within a 1,000 cc. beaker, which is filled with water and kept at the temperature desired for the stimulus. The volume of the water is such that the temperature does not change rapidly, and may easily be kept within one degree by the regulation of the burner or by the occasional addition of cold water or ice. A thermometer in the large beaker shows the temperature. The water to be injected is drawn in a pipette from the beaker, measured in a graduate, and poured immediately into the flask through a funnel-tube. The opening of a pinch-cock below the funnel-tube both admits the water and also allows the expulsion of air through the parallel tube. The pinch-cock is closed after the water has been poured in, and the water is allowed to stand for about a minute in order that it may resume the temperature of the bath. It is then forced into the stomach-tube or the rectal-tube by blowing air into the flask by means of a rubber bulb. This bulb is an ordinary syringe bulb, containing valves that prevent the return of the air. With a little practice the experimenter learns the squeeze that is necessary to force a given amount of water into the body with a minimum of accompanying air.

The same apparatus was used, without the temperature bath, for the introduction of chemical solutions and solids in suspension into the alimentary canal.

Mechanical stimulation of the alimentary tract was accomplished by the inflation of a rubber bladder: tubes 5, 6, and 11. The apparatus is shown in Fig. 2. The bladder on the end of the tube is pumped up directly by a syringe, and the pressure is measured by the height of a water column. The tube for the column is 150 cm. high; it was, however, seldom necessary to go above 70 cm. The body of air in the large bottle is intended to act as a cushion, and to prevent the minor fluctuations of pressure that occur with the pumping and with the action of peristalsis upon an inflated bladder. It is necessary to have the bottle of large cross-section, in order that the rise of water in the tube will not appreciably lower the level in the bottle and thus change the zero-point. The level of the water in the system is controlled by filling from the funnel-tube, which is ordinarily shut off by a pinch-cock. The observer lies upon a couch at such a height

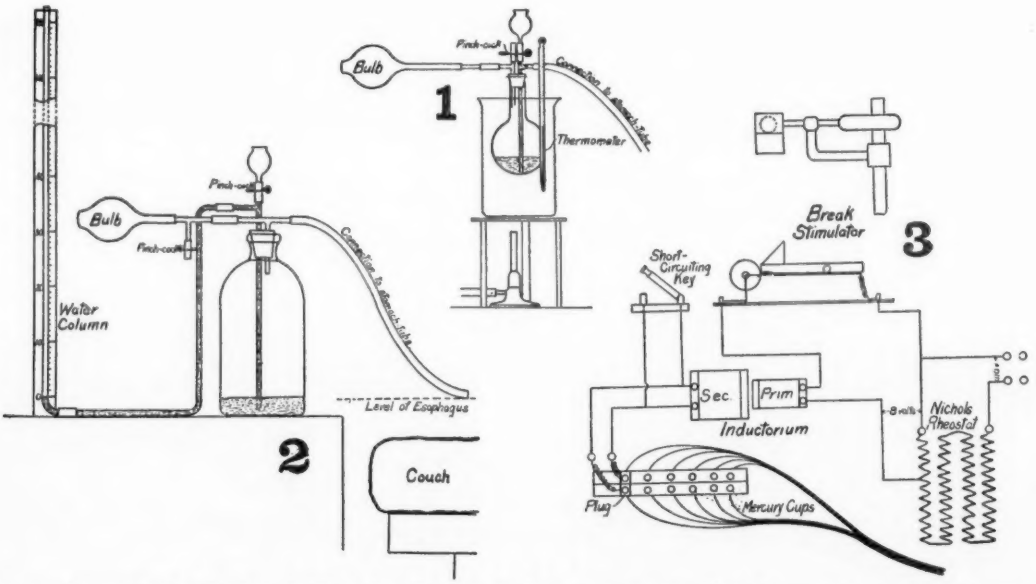


FIG. 1.—Apparatus for the introduction of a liquid at a given temperature under air pressure into the alimentary tract.

FIG. 2.—Apparatus for the inflation of a rubber bladder with air and the measurement of the pressure.

FIG. 3.—Apparatus for the bipolar electrical stimulation of the esophagus by break-shocks at 5 different points.

that the esophagus is approximately at the same level as the water in the bottle. A pinch-cock, connected with the stomach tube provides a means of release for the air after inflation.

In subsequent experiments, in which it was desired to control the rate of inflation, a bicycle pump was substituted for the rubber bulb. The pump was placed horizontally and in a convenient position and the piston rod put through a hole in a fixed metal plate. Two metal stops on the rod, one on each side of the plate, limited the length of stroke to 4 cm. The pump was worked by hand. As each stop came against the plate a metallic click resulted. The rate was controlled by the experimenter, who took care to make these clicks coincide with the clicks of a metronome. In a trial series it was found that the pressures obtained by an equal number of strokes were practically identical for rates varying from 15 to 75 per min.

For the esophagus and rectum it is probably fair to assume that the pressure measured by the water column is practically all exerted upon the tissues; that is to say, that the pressure is not reduced by the force necessary to expand the bladder alone. This assumption is approximately correct, because the bladder is confined within a tube and not allowed to expand in the normal spherical form. In the inflated bladder the only expansion is toward the end, and the internal pressure,—everywhere equal, and normal to the surface, by the law of hydrostatics,—must be transmitted almost entirely by the unexpanded side-walls of the rubber bag. Only to the extent that the esophagus or rectum stretches beyond the unexpanded diameter of the bladder is the measurement inexact. The error is probably greater in the stomach, where the bag takes more nearly the form that it would assume if it were inflated without constraint.

Fig. 3 shows the final form of the apparatus for electrical stimulation, as it was used with tube 8. The six pairs of contacts on the tube were connected with six pairs of mercury cups. The secondary of the induction coil was connected with a plug, made of a block of hard rubber bearing two pins which could be thrust simultaneously into a pair of mercury cups. In this manner the coil could be connected very readily to any pair of contacts on the tube. The induction coil is a 'standard'[23] coil, with a primary 13 cm. long and 10,000 turns in the secondary, manufactured by Zimmermann.[24] A removable core was kept within the primary throughout the trials. The scale for the secondary runs from zero to 20 cm. The coil was, however, calibrated in Kronecker units, and all values of inductive strength will be given in these units.[25]. Only break-shocks were used; the make was eliminated by a short-circuiting key, connected with the secondary. In the early experiments a telegraph key was used to break the circuit. This procedure, however, proved so inconstant that a Titchener sound-stimulator[26] was substituted. In this instrument a

[23] The dimensions of a standard induction coil are discussed by Martin, E. G., *The Measurement of Induction Shocks*, 1912, 88f.

[24] No. 1901 of Catalogue 20, 1908.

[25] For the method of calibration, see Kronecker, H., Ueber die Ermüdung und Erholung der Muskeln, *Arbeiten aus der physiol. Anstalt z. Leipzig*, 1871, 186; Martin, *op. cit.*, 16ff. The writer is indebted to the Dept. of Physiology of the Cornell Medical College for the use of their Kronecker coil and Martin key.

[26] For full description and figure of this instrument see Titchener, E. B., Experimental Psychology, vol. ii, pt. i, 1905, 153ff.

marble is dropped from a constant height (in this experiment, 22 cm.) upon a table, which rocks slightly about a pivot, breaking a circuit between a plate on the lower surface of the table and a pin set in the circumference of a wheel that is rotated by friction with the table. The break is very quick. Constancy is secured by the fact that contact can be set in only one way and that, after the release of the marble, the operation is entirely mechanical. The current employed was taken from a Nichols rheostat connected with a 110-volt direct current line. The open-circuit voltage of the primary circuit was 8 volts.

The same apparatus was used with tubes 7 and 12, with the omission of the mercury cups. The single contacts were connected directly with a pair of binding posts in circuit with the secondary coil.

The heating coil of tube 3 was connected directly with the 8-volt connections from the Nichols frame. A variable resistance was placed in the circuit with the secondary coil, but it was found unnecessary to use it. The intensity of stimulation depended in any case upon the length of time during which the current was allowed to flow; and the increase was found to be not too great with 8 volts, whereas with less than 8 volts the initial warming-up to the limen took too long.

It should perhaps be mentioned that an unsuccessful attempt was made to use unipolar stimulation. A sponge electrode of about 100 sq. cm. was attached to the arm, and the esophagus and stomach were stimulated. The sensations in the arm were noticeable long before any sensation from the alimentary canal could be observed. The intensity might have been increased to an amount sufficient to bring out the internal sensations; but the movement and the extreme discomfort induced in the arm would have proved too great a distraction, even if it would not have raised a question regarding the exact seat of the stimulation. It would have been possible to use a larger electrode; but the desire to confine the current as far as possible to the area under investigation forbade this attempt.[27]

General Procedure. Before the experimental work began, the observers had to learn to swallow the stomach tubes. They first tried the catheter and tube 1. They were instructed to swallow on the tube, to take it as one might a bolus. To aid in the swallowing, water was sometimes allowed to trickle slowly through the tube. As we have seen, the observers differed greatly. F from the first preferred a tube large enough to be so stiff that he could thrust it down rapidly. G, on the other hand, gulped the tube down very slowly inch by inch.

When the observers were first required to talk with the tube in place, they hesitated and spoke very little. Within an hour, however, they found themselves able to talk continu-

[27] Becher, however, did use unipolar stimulation with the whole arm immersed in water. He thought that, since he had proved the sensibility of the esophagus, there was but little doubt that the sensations originated there. *Zeitschrift*, 49, 352.

ously, with occasional pauses when the tube tickled the throat and started the gagging reflex. F was an exception to this rule, for he never became able to talk without vomiting, although he tried again and again. Work was thus slower with F than with the others, for it was necessary to remove the tube after each observation. The swallowing is the most uncomfortable (and sometimes painful) part of the operation. Hence in the case of F the series were not only delayed in the actual time consumed, but were also limited in the number of observations that could be taken in a single hour. For this reason fewer observations were in general taken with F than with the other observers.

Toward the end of the experiment, D and G could usually retain the tubes without serious discomfort. B had become so practised by continued work that it was not a great hardship for him to allow experimentation upon the esophagus for four hours in a single day, or to retain a tube continuously for two hours. He sometimes preferred keeping the tube in place, when going upon an errand in the laboratory, to the unpleasantness of having to swallow it again. Toward the end of the experiment he observed five hours a day for a week on the esophageal and rectal work combined. This experience was, it is true, somewhat upsetting; the explanation may lie, however, in the fact many of the stimuli were chemical.

All the experiments were performed with the observer lying supine upon a couch, which was adjusted to place him at the level of the apparatus. The stimulus tube was supported from the observer's mouth by a ring attached to a cord which ran over a pulley to a counterweight.

Localization. The position of the stimulus in the esophagus or stomach is recorded, throughout these experiments, as the number of centimeters from the teeth downward. The observer was instructed to keep the tube lying flat in the mouth, and not bunched up in the throat. In such a system, 15 cm. lies approximately at the junction of pharynx and esophagus; 20 cm., in the upper portion of the esophagus. For F, who is very tall (6 ft. 2 in.), this point must have been quite close to the pharynx. The gastric end of the esophagus lay between 40 and 45,—in B at about 45. This point was determined by the ease with which a bladder could be expanded within the tract, and by a radiograph (not reproduced in this paper), taken with the tube in, in which a shadow of the stomach, distended by gas, appeared. With the limits of the esophagus

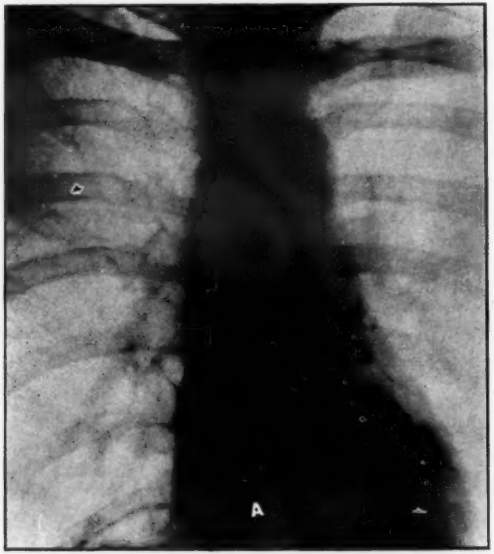


PLATE I

Radiograph of Observer B with esophageal tube in place. A = end of 40 cm. tube.

PLATE II

Coordinates for localization in relation to superficial anatomy. Observer B. Longitudinal scale corresponds to distance from the teeth of underlying points in the esophagus.

thus fairly defined, five points,—40, 35, 30, 25, and 20 centimeters respectively from the teeth,—were selected for investigation. They will be referred to by these numbers. The upper end of the esophagus is 20; the lower end, 40. There was no means of controlling the position of the end of the tube after it had entered the stomach. Ordinarily the setting 50 was taken for an observation upon the stomach. Occasionally 45, 55, and 60 were used.

The observers were required in all the trials to localize the sensations reported. In the cases of D and G the localization was made by reference to such definite anatomical parts as the larynx, clavicle, nipples, sternum, ribs, and umbilicus; or by indication upon a large outline diagram similar in form to the small ones shown in Figg. 4 *et seq.* The localization was recorded both verbally, and diagrammatically upon a smaller outline diagram which was placed on the record sheets by means of a rubber stamp. These general means of localization were supplemented, in the cases of B and F, by laying off the surface of the body in five-centimeter squares, as is shown in Plate II. Plate II is a photograph of B, and shows the anatomical relations. The longitudinal scale indicates the distance in cm. that a point at that level in the esophagus is from the teeth. The relation of the position of the tube, as measured from the teeth, to the surface of the body was determined for B by means of the radiograph of Plate I.[28] The transverse scale reads to left and right of the median axis. Thus the right nipple would be designated as 35R10, *i. e.*, 35 cm. down and 10 cm. to the right. The lines were sometimes marked off on the skin in grease pencil, the sternum, nipples and umbilicus being taken as basal points. More often however, the observer wore a 'localization map.' This 'map' consisted of a very tight-fitting undershirt, which was made to conform even more exactly to the body by elastic straps at the back and from front to back between the legs. The coördinates for localization were marked upon the shirt. Its adjustment could always be checked by reference to anatomical points.

[28] The clavicle and ribs are sufficiently clear in the radiograph and could be located with sufficient distinctness in the subject to establish the local relations. Between the measurements made upon the radiograph and those made upon the subject there was an average difference of 3.8%,—a difference which was neglected.

The following anatomical relations for B in terms of the scale show the significance of the numbers:

Top of larynx	12.5
Bottom of larynx	17.5
Level of clavicle	20.0
Nipples	35R10, 35L10
Lower level of sternum	40.0
Lower level of ribs	54.0
Umbilicus	58.0
Lower level of *symphysis pubis*	74.0
Base of penis	76.0

II. The Esophagus

1. *Thermal Stimulation*

Sensibility. The principal source of data upon the thermal sensibility of the esophagus is furnished by the series taken with tube 3. In these series, 35 cc. of water at different temperatures were injected under pressure into the esophagus. The method has been described above. Five positions of the tube (40, 35, 30, 25, and 20 cm.) were used with B and D, and three (40, 30, and 20 cm.) with F and G. In all these series the first observation was taken at the lowest position, and the tube was then pulled up; it was much more difficult for the observer, lying down, to have the tube pushed in than to have it pulled out. Temperatures of 0° C and 50° C were used with all the observers. With B many other temperatures were also used. In addition to these series, in which the position of the tube was varied while the temperature remained constant, a single series, in which the tube was at 30 cm. while the temperature was increased by small steps over the middle range of the temperature scale, was carried out upon each observer. The results (so far as a full introspective report, sometimes of as many as 200 words, can be reduced on its qualitative side to one or two words) are shown in Tables I, II, III, and IV for the four observers respectively. The observations recorded in these tables were taken in serial order from left to right for a single temperature, with the exception of those recorded in the column headed 30(II). This column gives the results of the series in which the temperature was increased with the tube at 30 cm. The plus sign (+) represents the successive appearance of the factors indicated.

The position of the end of the tube does not, of course, indicate the exact point of stimulation, since the water, after injection into the esophagus, continues downward. For this reason, any reports of temperature that occurred more than

TABLE I

Sensibility of esophagus to temperature. Observer B. Stimulus 25 cc. of water at given temperature introduced through tube 2. Position of end of tube recorded in cm. from teeth. Successive appearance of two factors is indicated by +.

Temp. of Water, °C.	Position of end of tube					
	40	35	30	30 (II)	25	20
0	cold	cold	cold		cold	cold
10	cold	cold	cold		cold	cold
20	cold		cold			cold
30	cold		cold			cold
35	cold		cold	cold		cold
37				cold		
38	cool		cool			cool
39				cool		
40	cool	cool	cool		cool	cool
	+warm	+warm	+warm		+warm	+warm
41				cool		
				+warm		
43				cool		
				+warm		
45				cool		
				+warm		
47				cool		
				+warm		
49				cold		
				+warm		
50	cold	cold	cold		cold	cold
	+heat	+warm	+warm		+warm	+warm
60	heat	cold	cold		cold	cold
		+heat	+heat		+heat	+heat
70	cold		cold			cold
	+heat		+heat			+heat
						+pain
80			heat			
			+pain			

a few seconds after the beginning of the stimulation have been excluded from the tables. The water must have spread out, and have approached body-temperature, very rapidly after leaving the tube. The most intense sensation would thus probably occur somewhere in the neighborhood of the end of the tube. The generalization that the sensations reported depend upon the stimulation of the region of the esophagus at the end of the tube cannot be made with any great rigor. It is, however, supported to some extent by certain facts. (1) The esophagus can be shown to be sensitive to tempera-

TABLE II

Sensibility of esophagus to temperature. Observer D. Same as Table I.

Temp. of Water, °C.	Position of end of tube					
	40	35	30	30 (II)	25	20
0	cold	cold	cold		cold	cold
20				cold		
31				cool		
35				cool		
39				cool?		
43				warm		
47				warm		
50	warm	cool +warm	cool +warm		cool +warm	cool +warm
51				warm		
55				warm		
59				warm		
63				warm		
67				heat		
71				heat		

TABLE III

Sensibility of esophagus to temperature. Observer F. Same as Table I.

Temp. of Water, °C.	Position of end of tube			
	40	30	30 (II)	20
0	cold	cold	cold	cold
31			cold	
35			cold?	
39			nothing	
43			nothing	
47			warm	
50	warm	warm		warm
60	warm	warm		warm

ture, throughout its length, by other means (tubes 3 and 4) which will be described later, and has been shown to be thus sensitive by other investigators. The results obtained by the present method agree substantially with those obtained by the other methods. (2) The esophagus appears to be more sensitive toward the upper end than the lower. This difference

TABLE IV

Sensibility of esophagus to temperature. Observer G. Same as Table I.

Temp. of Water, °C.	Position of end of tube			
	40	30	30 (II)	20
0	cold	cold		cold
23			cold	
27			cold	
31			cold	
35			cold	
39			cool	
43			cool	
47			cool +warm	
50	nothing	warm		warm
51			cool +warm	

in sensitivity must be real, for, if the sensation were due to stimulation by water at a lower level, the upper esophagus would appear less sensitive than the lower, since, with the tube in the higher position, the water would have farther to go and would hence have assumed a more nearly neutral temperature before it had reached a sensitive region.

The following points may be noted in connection with the tables.

The esophagus appears to be sensitive throughout its length to cold at 0° C and to warmth at 50° C, though in a single instance G had no temperature sensation at 50°. Our results here are in perfect accord with those of Becher[29] and of Hertz, Cook and Schlesinger.[30]

A noteworthy fact appears in the report of B (Table I). Cool or cold sensations are reported not only for the stimuli below body-temperature, but also for all but two of the stimuli above body-temperature. In the fourteen reports of warm, every warmth is preceded by coolness or cold; in the ten reports of heat, cold precedes in every case but two. D and G occasionally, but by no means universally, find cool sensations preceding the warm. With D the cool sensations occur four times out of thirteen; with G, twice out of four. F, at the other extreme from B, never gets coolness before

[29] *Zeitschrift*, 49, 349f.
[30] *Op. cit.*, 483; Hertz, *op. cit.*, 6.

warmth. There can be no doubt about these cold sensations; they are often more clear-cut and definite than the warmth which they precede. We shall return to their consideration presently.

From inspection of the Tables it is possible to write the approximate liminal values for cold, warmth, heat, and painful heat, which are set up at a point in the middle of the esophagus:

	B	D	F	G
Cold[31]	37	35	35	35
Warmth	40	43	47	47
Heat	60	67	..	..
Painful heat	80	..	..	..

At the lower end of the esophagus in observer B the limen for cold is higher, and for heat lower, than is the case in the middle of the esophagus.

In order to demonstrate more conclusively that the whole esophagus is sensitive to temperature, tubes 3 and 4 were used with observer B. The heating coil of tube 3 was allowed to become hot at the five positions usually selected in the esophogus. In all cases there appeared a sensation of warmth, followed by heat. The heat developed later into pain except at the extreme lower end of the esophagus (40 cm.). Here, too, it might have passed over into pain had the circuit been kept closed for a longer period; B feared, however, to allow the current to pass for more than fifteen seconds under the poor conditions of radiation that probably exist in the esophagus, since the coil would have become very hot and might have injured the tissue. The use of the coil demonstrates the sensibility of the esophagus easily and unequivocally. It is not adapted to quantitative determinations, as it cannot be calibrated without reference to the conditions of conduction and convection that obtain in any particular case; there is theoretically no limit to the temperature that a coil, to which electrical energy is constantly being supplied, may reach, except as a limit is established by the rate of energy-conduction away from the coil under any given set of conditions.

More satisfactory results (see Table V) were obtained with observer B from tube 4. In these trials a current of water (50 cc. altogether) was forced by means of the apparatus shown in Fig. 1 through the brass coil at the end of the stimulus tube. The esophagus proved everywhere sensitive to cold, warmth, and painful heat, with the single exception that pain only was felt for the extreme cold at the upper end of the esophagus.

This stimulus tube has the advantage over that used for the greater number of our experiments that it confines the stimulation to a small area until such time as conduction takes place through the heavy walls of the tube. It has, however, the disadvantage of affording great discomfort to the observers. In fact, after an attempt had been

[31] The figures for 'cold' are temperatures at which a definite, intense cold sensation is felt. 'Coolness' is seen to be reported for higher temperatures.

TABLE V

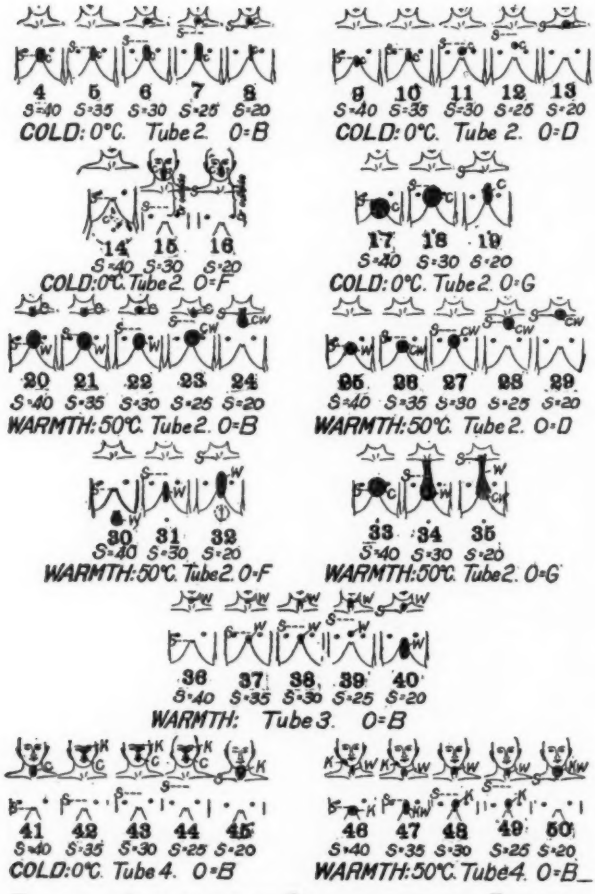
Sensibility of esophagus to temperature. Observer B. Stimulus: current of water at a given temperature passed through the brass coil of tube 4. 50 cc. of water are forced through under pressure, in each observation.

Temp. of Water, °C.	Position of stimulus coil				
	40	35	30	25	20
0	cold	cold	cold	cold	cold
50	warm	warm	warm	warm	warm
60	heat +pain	warm +pain	heat +pain	heat +pain	heat +pain

made to use it with F and D, the resulting soreness of the esophagus persisted for so long a time that it was considered inadvisable either to repeat the attempt on these observers or to try to use the tube with G, who has a small pharynx. The experimenter tried to construct an apparatus which should use as stimulus a loop of light rubber tubing, the ends of which were brought up through the heavy outside tube. He was unable to construct a loop which would not kink, so as to cut off the flow almost completely, even when the water was under considerable pressure. When heavier rubber was used for the inner tube, the loop changed in temperature almost as slowly as the outside tube so that the arrangement seemed scarcely preferable to tube 2. The substitution of a thin rubber sack for the loop also proved unsatisfactory, because the sack when full of water initiated peristalsis and gave rise to distracting pressure sensations.

It is tempting to call the cool and cold sensations dependent upon stimuli above body-temperature 'paradoxical.' We have seen (p. 17), however, that for observer B all stimuli from 0° C to 70° C give either coolness or cold, that there is no *Nullpunktstemperatur*. The same state of affairs occurs when the end of the tube is held in the hand; coolness precedes warmth, for the mass of the tube itself is such that the first volume of water is always cooled down to a point sufficiently below body-temperature to give rise to a sensation of coolness in the normal way. The results for tube 2 thus come into accord with those for tubes 3 and 4, with which no pure warmth sensations were obtained. The difference between observer B, who always gets coolness with warm water, and F, who never does so, may be entirely one of sensitivity. D and G are, perhaps, intermediate.

Is there then no paradoxical cold? The appearance of heat for stimuli more intense than those which give warm and with all three tubes would, on the theory that heat implies cold (von Frey), indicate a paradoxical cold. It is noteworthy that with B (Table I) the initial sensation for the higher temperatures is cold, that is to say, coolness generally precedes warmth and cold heat. It may be that the cold organs respond more rapidly than do the warm organs, as is the case when one thrusts one's hands into hot water, and that this cold is a true paradoxical cold. That it would not be felt with tubes 3 and 4 is natural enough, since these tubes present a stimulus which becomes warm very gradually.



FIGG. 4-50.—LOCALIZATION OF TEMPERATURE IN THE ESOPHAGUS
O = Observer; S = level of stimulus (cm. from teeth).
c = cold; k = pain; w = warmth.

Localization. The localization of the sensation of temperature for typical cases is charted in Figg. 4 to 50.

Localization of cold is shown for four observers in Figg. 4 to 19. Individual differences are apparent. B, for example,

localizes cold both in the throat and in the region of the sternum. G finds cold in the same places as the lower cold of B, but does not refer it to the throat. D tends to localize the stimuli near the sternum, except those at the upper end of the esophagus. F finds localization very difficult for the higher positions of the stimulus; he thinks that the cold may lie in the head or lower down, but he cannot make sure. "My first tendency was to connect the cold with the tactual feeling in the upper part of my throat. This did not seem right. Then I felt the cold sensations outside my body in front of me. They seem to move up and down." With tube 4 at 0° C, B refers cold universally to the throat. He does not find any sensation of temperature in the region of the stomach.

In general it may be said that the sensations, following cold stimulation are referred either toward the region of the stomach or toward the throat; that is to say, they are hardly ever localized in the upper portion of the chest.

The localization of warmth by B, D, and G is similar to that of cold. B always experiences a cold sensation preceding the warmth; the cold is felt in the neck, and the warmth at the sternum. G feels cold at the level of the sternum, but warmth frequently extends up beyond it. D localizes the preliminary cold and warmth in the same place. His localization corresponds more nearly with the position of the stimulus than that of the other observers. F refers the warmth to the abdomen or sternum, never to a higher position. He does not get sensations of cold from warm stimuli.

With the electric heating coil (tube 3), and with the brass coil for water (tube 4), B localizes the sensations in very much the same places as with the single tube (2); but in neither case does he feel cold. With the electric heating coil, the throat sensations and the sensations lower down were those of warmth. With the water coil, the throat sensations were warm, while the sensations lower down consisted of a dull pain or ache.

In general we may say that the warmth sensations tend to be localized in the region of the sternum or below, when warm water is introduced through a straight tube, although numerous exceptions occur with other methods of procedure (Figg. 36-40, 46-50). The cold aroused by warm stimuli is referred to the neck by B, to the region of the sternum by G, to the same place as the warmth by D, and is not reported by F.

Quality of Sensation. The reports of the observers indicate the character of the complexes aroused by thermal stimulation. The following excerpts apply for 0° C and tube 2.

B, 40 cm. "Besides usual bubbly sensations, cold developed quite definitely in long strip extending about 2 inches on either side of sternum. It lasted with some slight fluctuation to the end, by which I mean, the place where all relevant sensations stop. Considerably above this a very keen fresh cold came in,—like eating peppermint. I intended at first to localize this in my throat, but when I attended to throat it seemed as if the cold must be farther down. Then I tried to locate it with my finger, but could not decide whether it was in my throat or opposite my clavicle or considerably lower down. The thing seemed peculiarly evanescent."

B, 35 cm. "Lots of cold like that described last time. Then very intense cold extending all the way down the throat to my sternum, but although it seems continuous it seems to skip my neck. I visualize these sensations as continuous when I think of them as inside, but there seem to be about four inches of neck missing when I think of them with reference to the surface of the body."

D, 35 cm. "Cold sensations very noticeable; seem to be combined with a lump of pressure."

D, 20 cm. "Extremely cold, and sort of refreshing; quite high up in the throat."

F, 40 cm. "Moderately intense cold, which seems to be in front of bubbly sensations at body wall. Nothing is in front of my body. There was another cold inside, and a third higher up."

F, 30 cm. "My first tendency with the cold was to connect it with the tactual feeling of the tube in the upper part of my throat where I feel the tube most. This did not seem right. Then I felt the cold sensations outside my body in front of me. They seem to move up and down; and then, because I knew the length of the tube in the esophagus was not great, I tried to localize them down below the sternum; but this did not seem right either. The cold is farther in front, perhaps even outside the body."

G, 30 cm. "Auditory and pressure sensations in stomach—bubbly sensations. Cold came blended with these sensations, but did not stand out clearly at first. Afterwards, however, it became clearer. It lasts a long time; I still feel a tiny bit of cold in my stomach."

G, 20 cm. "Much the same as before. Cold not different on edges but different in midline. At first it was mixed in with stomach sensations. It stood out quite clearly after the beginning. At the end there was just a pattern of the cold sensations left."

With the water coil at 0° C and tube 4, B reports as follows:

B, 40 cm. "Very intense cold in throat and at top of pharynx. It develops and gets very intense rapidly. Coldness on the lips and tongue (probably from the conduction through the walls of the tube) come very much later. No sensations below clavicle."

B, 30 cm. "Intense cold in tiny little spots way high up above the base of the uvula."

B, 25 cm. "Cold lasts right through and becomes larger and 'achy' toward the end. I can feel this dull ache up in the region above the uvula at the same time that I feel coolness in the mouth from the tube. By a shift of attention I can also localize the cold back of the bridge of the nose, which is where I localize the intense pains which come from eating ice-cream too fast."

There is on the basis of the reports no reason to suppose that cold from the esophagus is qualitatively different from cutaneous cold. The sensations are generally referred to a large area, and the indefiniteness often ascribed to them seems to be principally a matter of difficulty of localization of this area. Either the region is not exactly placed, or its boundaries are poorly defined, or it is localized differently by different methods of judgment. B, for example, reports an indefinite sensation which he tended to localize in one place with visual imagery and in another by arm-kinesthesis. The indefiniteness is probably also a matter of intensity. Weak sensations covering a large area are apt to be called indefinite. Besides this indefiniteness, the dull pains that frequently accompany the esophageal colds are apt to give a specific qualitative coloring to the experience. Careful analysis, however, seems to separate the 'achy' cold into two components, the one of which is not unlike the cold of the skin.

The character of the sensation aroused by warm stimuli appears in the following quotations.

B, 40 cm. "Very different and quite intense cold sensations well up in region of clavicle. This cold is a little spot about two inches long and one inch wide, although the boundaries were not definitely defined. It reached maximum intensity rather quickly, though not suddenly, and died down considerably more slowly. Then a short period with no cold in it, but possibly a very weak warmth. Later the warm sensation became definite, reaching a maximum much more slowly than did the cold. At maximum intensity it seemed very slightly hot,—at least there was some qualitative change in the direction of getting more clear-cut and compact."

B, 30 cm. "Cold in throat early as before; then a less definitely localized cold lower down; followed by warmth. The warmth is quite diffuse, and not like cutaneous warmth, but as if something unpleasant were set on a background of cutaneous warmth. It is not just like heat because this something is not so sharp, definite, and clear-cut."

B, 20 cm. "This time the cold went over into warmth by way of heat. Instead of there being a low place in the temporal course between the cold and the warmth, there was a high place. I can distinguish the cold in this heat. Heat becomes less intense and then turns to intense warmth which dies out fairly rapidly."

D did not discriminate qualitatively between warmth from the skin and warmth from the esophagus.

F, 40 cm. F first describes "bubbly sensations" which are due to the injection of water through the tube and which were localized in the center of the body about the umbilicus. He finds warmth in front of the sensations and covering them.

F, 30 cm. F finds warmth as part of a complex of pressure and warmth. "The warmth is well forward in my body, very hard to localize. If you are going to get it as exactly as possible, you would say it was less than an inch below the surface of the body. No particular thickness to the warmth, which is a thin sheet."

G, 30 cm. "Weak, bubbly, followed by warm pattern. Localized warmest part just below sternum extending up into sternum. Strange to say there was also warmth from clavicle down to stomach. Wondered if I got it way up in top of throat, but decided I did not."

G, 20 cm. "Bubbly pattern, followed by weak cold in stomach; followed by and overlaid by warmth—warmer than before localized—strongest in clavicle region. Also some warmth below though this was not intense. Should say attention was on cool at first; then on definite warmth above; and then on indefinite warmth below."

It appears, then, that if any difference in quality exists between warmth from the esophagus and warmth from the skin, it is neither great nor obvious. The lower warmths, like the colds in the same region, are frequently described as indefinite or vague, but no observer calls this a qualitative difference, with the single exception (noted above) that B suggests there may be some "qualitative change in the direction of getting more clear-cut and compact."

We cannot enter here into a discussion of the possibility of a description of qualities in terms that ordinarily have an extensive connotation (*e. g.*, 'granular pressure'). As regards the validity of the qualitative distinction hinted at by B, it may be remarked that, although B found the warmth in the stomach vague and indefinite, he was able under special conditions to equate it to warmth of the skin (see p. 44). Further introspections indicate that the warmth at the lower end of the esophagus was more like heat. Of the exact analysis of heat, B has never been able to make up his mind. In it, warmth is usually clear for him. Cold is sometimes, but rarely, discernible; although cold differs from warmth in much the same way that heat does, *i. e.*, in definiteness and clear-cutness. His introspection, however, does not lead him to believe that the introduction of cold alone will change warmth to heat. When warmth becomes heat, it seems as if a 'stinging' sensation, which belongs to the family of pains, were added to it; and the existence of such a sensation is supported by the fact that it seems to occur in isolation when the glans penis is stimulated by warmth. It is possible, then, that for B stimulation of the lower esophagus by warmth tends to bring out, along with warm sensations, some sort of pain complex, which is perhaps not unlike a factor in the typical pattern for heat. This painful addition might account for the 'unpleasant warmth' described by Head as the characteristic internal warmth. In any event there seems to be no question but that the warmth from the upper esophagus is like that of the skin. All observers find them similar in character. This upper warmth passes often into a normal heat in the usual way.

Thermal Pain. Both cold and warm stimuli in the esophagus sometimes give rise to pain. The pain is frequently localized with the sensation of temperature. There are, however, many cases in which pain is referred to the head (see Figg. 42-47). The localization of these pains in the head is very indefinite. B nearly always finds them deep in. They

seem to alter their position for him, however, with alteration of direction of attention; *e. g.*, when visualized from the front they appear to be behind the bridge of the nose, when visualized from the mouth they appear to be up above the uvula. Sometimes, especially with warmth and nausea, an indefinite pain occurs in the jaw. It is hardly possible to place this pain more specifically than to tell which side it affects. The thermal pains are of the indefinite ache-like character of many of the organic pains. In one form they are familiar to most people, as the pains experienced in the head when eating ice-cream too rapidly.

The prevalence of 'ice-cream pains' was determined by questioning a class of forty-five students. Thirty-eight were accustomed to feel the pains when they ate ice-cream too quickly. The seven who never felt the pains did not characterize themselves as 'rapid eaters.' Those who were accustomed to them localized them variously as follows: head (2); front of head (3); head above nose, forehead over eyes, between eyes, bridge of nose, above nose, base of nose (23); temples (3); roof of mouth, soft palate (4); jaws (1); throat (1); below throat (1); intestine (1).

These pains have been explained as resulting from the stimulation of the esophageal and gastric branches of the vagus. The former ends in the esophageal plexus and lies close to the esophagus especially at the lower half. This distribution accords with the fact that the pains occur more readily upon the stimulation of the lower end than they do when the middle region of the esophagus is excited. The pharyngeal branch of the vagus may be responsible for the pains originating in the throat. The reference of the pains to the head may in some way be due to the fact that two branches of the vagus (meningeal and auricular) are distributed in this region.[32]

2. *Mechanical Stimulation*

Sensibility. The sensibility of the esophagus to mechanical stimulation was studied by inflation of the rubber bladder of tube 5. The apparatus has been described (see Fig. 2).

In Table VI are shown, in terms of cm. of water, the intensities of pressure required to produce in the first place a just noticeable sensation of pressure, and in the second place a just noticeable sensation of pain. The two values of pressure shown for B represent two separate series. The results are not exact to less than 5 cm.; because the column of water, owing to the fluctuations caused by peristalsis and the tendency of most observers to squeeze the tube involuntarily between the lips, did not remain constant.

The Table shows, not only that the esophagus is sensitive

[32] Indigestion is said to be sometimes accompanied by a ringing in the ears.

to pressure and to the pain of distension, but also that its sensitivity increases toward the upper end.[33]

TABLE VI

Sensibility of the esophagus to pressure and to pain. Figures show the approximate intensity of pressure in cm. of water necessary to arouse a just noticeable sensation. Tube 5.

Position of Stimulus, cm.	Pressure				Pain			
	Observers				Observers			
	B	D	F	G	B	D	F	G
40	31, 21	30	30	35	40	50	40	40
35	15, 15	30		25	45	40		37
30	20, 26	25	30	25	40	45	40	32
25	12, 21	20	25	20	37	40	40	40
20	13, 25	25		5	35	40		20

The statement is ordinarily made that the limens for the sensations dependent upon the distension of the alimentary tract vary with the rate of distension. To test this assertion the rate of inflation was controlled by the use of the pump (p. 10). Five trials were taken at 30 cm. with each of five rates ranging from 15 to 180 half-strokes per min. The average liminal intensities for pressure expressed in cm. of water are as follows:

Rate (half-strokes per min.)	15	30	60	120	180
Average of 5 trials.....	33.3	28.3	23.3	21.9	19.5
Mean variation	4.1	4.8	7.8	4.1	1.0

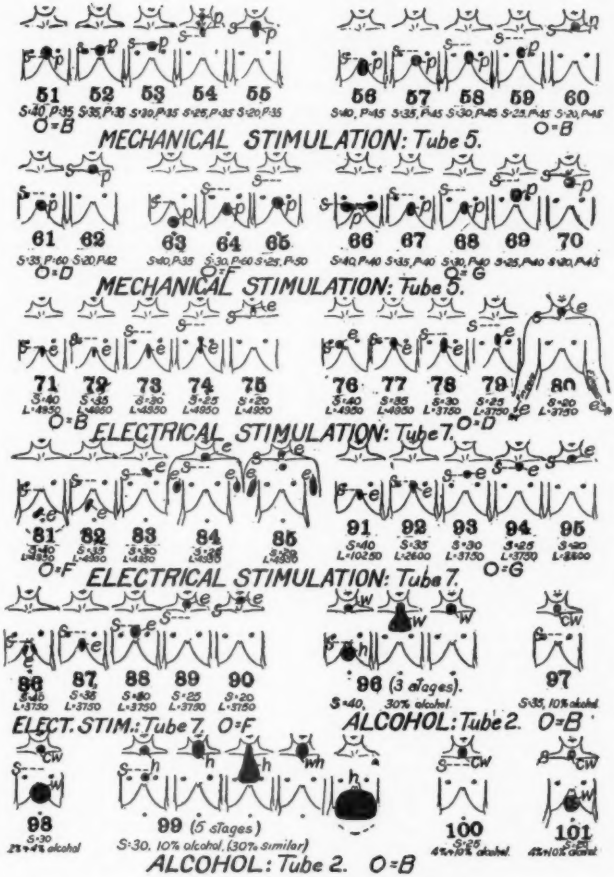
Although the M. V.'s are large, there can be no doubt but that the limen for pressure is decreased with an increase in the rate of distension.

The liminal values for pain from the inflation of the esophagus at 30 cm. are:

Rate (half-strokes per min.).....	30	60	120	180
Average of 5 trials.....	42.3	44.8	43.5	41.8
Mean variation	4.3	3.8	6.0	4.7

There is no evidence that the limen for pain is dependent upon the rate of inflation. This conclusion must not be drawn too

[33] Hertz (*op. cit.*, 16) finds a limen equivalent to 60-70 cm. of water for the sensations of 'fulness.' He denies tactile sensibility (p. 3). Becher (*Zeitschrift*, 49, 351) reports that the esophagus of his observer was sensitive to both pressure and contact.


MECHANICAL STIMULATION: Tube 5.
MECHANICAL STIMULATION: Tube 5.
ELECTRICAL STIMULATION: Tube 7.
ELECTRICAL STIMULATION: Tube 7.
ELECT. STIM.: Tube 7. O=F
ALCOHOL: Tube 2. O=B
ALCOHOL: Tube 2. O=B

FIGG. 51-101.—LOCALIZATION OF MECHANICAL, ELECTRICAL, AND CHEMICAL STIMULI IN THE ESOPHAGUS

O = Observer; L = intensity of electric shock (Kronecker units); P = maximum intensity of pressure (cm. of water); S = level of stimulus (cm. from teeth).

c = cold; e = electric shock; h = heat; p = pressure; w = warmth.

rigorously, however. At these high pressures peristalsis is induced so vigorously[34] that the water column in the manometer varies rapidly, making an exact reading difficult or impossible. The peristalsis, moreover, draws upon the tube and the resulting sensations are very distracting. Further, the sensational complex is so complicated that the appearance of an additional factor in the form of a just noticeable pain is inherently difficult to determine introspectively.

Localization. The localization of these complexes of pressure and pain is shown in Figg. 51-70. The same phenomenon recurs that was observed with temperature, *i. e.*, a tendency to refer sensations in both directions away from the upper portion of the chest. A difference appears, however, in the fact that none of the pressure sensations are referred to points above the actual place of stimulation. In B and D, the two upper points are sometimes localized correctly. The lower points, although varying in height with the height of the stimulus, are placed very much too low. All the localizations of G and F are below the stimulus. F makes almost as great errors for the lowest point of the esophagus as for the upper points. Some of his localizations (not shown on the figures) were as low as the umbilicus.

Quality of Sensation. The character of the experience resulting from mechanical stimulation is shown by the following extracts from the reports of the observers.

B, 40 cm. Max. press. = 35. "Pressure first felt as below clavicle, 6 to 7 cm., or as up in top of back of throat,—the usual confusion. As pressure got more intense, it got more unpleasant. Suddenly I realized that this feeling was exactly the thing that gives warning when one is about to vomit. It is in nausea, but so far as I can tell is purely pressure."

B, 40 cm. Max. press. = 45. "At first pressure, very much like a dull pressure, *i. e.* subcutaneous pressure; very soon, however, it becomes dull and 'achy.' The ache is not definitely pain. It is the peculiar characteristic of these internal pressures, and seems more to color them than to be a separate component. The complex is something like the ache of muscular pressure, but more definite than I have ever felt in muscular pressure, unless it be in fatigue."

D, 40 cm. Max. press. = 35. "At first pressure; then felt sort of bloaty. Fullness contrasts with my feeling afterwards when the air is released."

D, 35 cm. Max. press. = 60. "Felt definite pressure below sternum and a tendency for the tube to pull down. Pressure then got more intense and 'achy;' resembled something that swallows hard."

F, 40 cm. Max. press. = 35. "Very pressury sensations in a small

[34] *Cf.* Hertz, *op. cit.*, 17.

lump, like a kind of a ball of muscular pressure. Intensity rises gradually; the sensations did not flash out."

F, 30 cm. Max. press. = 45. "Diffuse. This time the sensations were not so pressury; quite a good deal more intense; later the sensations went way up to the head and around and up to the nose." Q. "Pressure sensations?" A. "No. Much livelier than muscular pressure; more the quality that muscular pressure has when it gets quite sore and unpleasant; thinner and finer in diffusion than muscular."

F, 30 cm. Max. press. = 20. "Ball of muscular pressure below end of sternum stayed a little while and then disappeared."

G, 40 cm. Max. press. = 40. "At first pressure with slight coloring of pain localized in the ribs. Very funny. Both sides, sometimes front. A little later, the pain constituent becomes prominent, although not so very prominent."

G, 30 cm. Max. press. = 40. "At first pressure at sternum at middle line. Then pressure became more intense and a little crampy. Later it became very crampy and unpleasant."

G states after the series that "all these pressures are dull, heavy pressures like muscular pressure, not cutaneous in quality."

The introspections indicate that the esophagus mediates a pressure, which is like a muscular pressure, if not identical with it. In order to make the comparison more definite, the experimenter tried giving the esophageal complex and a complex produced by deep pressure on the arm simultaneously and in rapid succession. The observer swallowed the tube to 30 cm. Then the skin of the upper forearm was anesthetised by spraying with ether. A pressure, at the anesthetised portion, was produced by a Cattell algometer. Two sorts of trials were made; one, in which the intensity of the pressure on the arm was sufficient to bring out deep pain, and in which the amount of inflation of the bladder was sufficient to elicit the ache-like "crampy sensations;" and another, in which the intensities were weak enough to produce only pressure sensations in both esophagus and arm.

D, in comparing esophageal pressure of 60 cm. of water with an arm pressure of 3.5 kg., thought that the pressure sensations were identical in quality, although the one on the arm was more definite and less diffuse. The pains, he found, were both different from cutaneous pains. He was not sure whether they were alike. The pain from the arm was less like an 'achy' pain, was harder. His introspection upon the weaker intensities (30 cm. of water and 2 kg.) bears out his earlier report that the pressure sensations are alike in quality, although differing in extensive and intensive patterns.

F, after comparing 60 cm. of water in the esophagus with 3.8 kg., reports: "I can not find the slightest difference so far as the quality is concerned. The pain strains were of slightly greater intensity in the esophagus and were of wider extent."

After observing the weaker intensities (32 cm. of water and 1.8 kg.), F reported: "I do not know what I mean by thick and dull, but

certainly the arm sensation was much thicker and duller than the sensation in the esophagus, had much more body to it, was heavier. It seems to me as if the pressure in the esophagus went through a series of stages, from the weak pressure right up through the strainy, deep-pain thing, in more or less of a straight line, although you can tell when the pain comes; whereas in the arm you can pick out the place where the new quality appears."

G, after being practised in discrimination of kinesthetic sensations in the arm, is asked to compare 50 cm. of water in the esophagus with 2.5 kg. She reports: "Muscular pressure in chest with tendinous strain, forming a crampy complex. In the arm there was first muscular pressure. Then pain gradually began to be fused as it got stronger. The complex in chest was crampy in character."

With the weaker intensities (25 cm. of water and 1.2 kg.), G says: "In my arm I got a sort of muscular pressure; localized within; thick, not areal. In the chest there was a scrapy lump before blowing up. This became less, while muscular pressure gradually became clearer. At first it seemed like the pressure from the arm, and then became more distinctly muscular and, I think, tendinous. When the tendinous sensation came, it had a very slightly crampy character, a sort of strain."

It appears that D and F identify the pressure components of the esophageal complex unequivocally with muscular pressure in the arm. G observes that the esophageal pressure is more crampy in character, and is inclined to suppose that it must be of the tendinous order of pressures. We have already seen that F had difficulty in deciding whether the internal pressure was painful. His difficulty arose from the fact that the muscular pressure seemed to go over without sensible break into a painful pressure. It may be that G has the same difficulty. It is more probable, however, that she failed to experience the muscular pressure in isolation because (as she insists in her reports) the painful sensations from the end of the tube were never absent. G does not herself call the arm sensations muscular, but designates them as "deep pressure," in distinction from "muscular pressure," which approaches tendinous strain and which is found in the esophagus. B did not make the comparison between arm and esophageal pressures, but was convinced throughout that the pressure from the esophagus was of the same general order as muscular pressure.

All the observers agree that there is no qualitative difference between the sensations of pain from the esophagus and those from the arm, although they find very different extensive and intensive patterns. The pain in the arm is sharply defined and intense throughout; it is also often more definitely localized. The pain in the esophagus is much less definite in extent and less uniform in intensity.

3. *Electrical Stimulation.*

Sensibility. In these series, bipolar electrical stimulation (tube 7) was used in the manner and under the conditions described above (p. 10). All the observers were sensitive to electrical shock throughout the length of the esophagus. Four columns of Table VII show the approximate minimal intensities of break-shock required to produce a sensation. It will be seen that the sensitivity of the esophagus increases from the lower end up. The very small value for G at 40 cm. is possibly due to projection of the lower pole below the lower end of the esophagus. G's esophagus probably does not in any event extend below 41 cm. from the teeth; and since the measurements were taken to a point midway between the two poles, the lower pole would have been more than 40 cm. down.

TABLE VII

Sensibility of the esophagus to electrical stimulation. Figures show the intensities of shock in Kronecker units (8 volts primary circuit) which brought out a just noticeable sensation. The last column gives a set of equivalent intensities for observer B.

Position of Stimulus	Intensity of induction shock in Kronecker units				
	For just noticeable sensation				For equivalent intensities of sensation
	Observers				Observer
	B	D	F	G	B
40	3750	3750	3750	10250	3400
35	2600	3750	3750	2600	2800
30	2600	3750	3750	2600	2350
25	1500	1500	700	1500	2350
20	700	1500	700	700	1300

In a subsequent series for localization (p. 36), the writer found it necessary to determine the equivalent intensity of electrical shock for the different parts of the esophagus. This determination was made with the apparatus for localization without knowledge (tube 8; see p. 7). A setting of the secondary coil at 2,800 units was found to give a shock which was fairly intense but not uncomfortable. Accordingly, 2,800

at 35 cm. was selected as a standard. Series were taken in which the other points were compared with this standard. In every case, the intensity of the point compared was varied from weaker to stronger, and also from stronger to weaker. The order of variable and standard was reversed at random, so that the observer was not prejudiced by knowledge of conditions. The variations were made by 2 mm. steps upon the scale of the inductorium (equivalent to about 100 Kronecker units). In practically all cases, the points of equality in the ascending and descending series agreed, and were selected as the stimulus value corresponding to equivalent intensities. They are given in the last column of Table VII. They indicate further that the sensibility of the esophagus increases toward the upper end; and that the increase is rapid at both ends and very small in the middle region.

The large number of units required to produce a just noticeable sensation indicates the high absolute limen of sensitivity of the esophagus. We have noted above (p. 11), in discussing the impossibility of using unipolar stimulation, that the periphery, even with a relatively larger electrode, is very much more sensitive than is the esophagus. This statement applies only to the lower portions of the esophagus. The upper portions are probably as sensitive as the skin, if not more so. A just noticeable sensation upon the skin occurs when the coil is set at 1,500, and the fingers are moistened and held firmly on the terminals.

Localization. The localization of these sensations is shown in Figg. 71-95. With B and D the reference seems to be much the same as for pressure and temperature (Figg. 9-13). The error of localization is such that the indicated points do not lie upon the upper chest, but upon places above or below. F, with weak stimuli, gives a similar result. G shows a remarkably small error, even indicating two positions on the chest correctly.

With intense stimuli in the upper region of the esophagus F notices sensations in the upper arm. D also refers a moderately intense stimulus at the same place to the fingers. With intense stimuli in this region B regularly feels shocks in the fingers and in portions of the arm (most frequently in the upper arm, less frequently in the shoulders).

It may be objected that the spread of the current, and the stimulation by an electrical shock of nerve trunks at points other than their terminations, render this method ill-adapted

for a study of localization; and it may be added that some of the shocks are obviously localized far from the seat of stimulation. But the shocks in the arm are so different in character from the pressure-like sensation elicited by a weak shock in the esophagus that confusion is unlikely to occur so long as the stimuli are weak.

Quality of Sensation. The following extracts from the reports show the quality of sensation brought out by a weak electrical shock. Intense shocks gave complexes quite as difficult to describe as those aroused by intense shocks on the surface of the body.

B, 40 cm. Coil = 4950. "A different sort of catchy pressure, dull and 'achy.' I think that I have exactly the same sensation in swallowing something too large."

B, 20 cm. Coil = 4950. "Pressure faint and tiny; finer, narrower, brighter, and more cutaneously alive than before; more definite and also more like other peripheral experiences."

D, 40 cm. Coil = 4950. "Pressure, somehow mingled with pain. A catch, rather unpleasant."

D, 30 cm. Coil = 3750. "Quite a bit like swallowing something hard; seemed to extend up and down for an inch and a half."

D, 20 cm. Coil = 3750. "Different from all the rest; more like an electrical shock." Q. "Can you say anything about the quality?" A. "Like a very hard pressure; something like being up against a corner; a little bit of numbness."

F, 40 cm. Coil = 3750. "Sensation of very weak absolute intensity. Fine lines of sensation, rather than an area of quality; like the jerky muscular sensation that you get when a muscle is suddenly contracted electrically or otherwise: something very close to strain in it; because it is strain. I think there is a background of sensations spread over the whole area, but it is very weak indeed and very diffuse. I can hardly be sure it is there,—like the ticking of a watch far away. The quality is dull. It seems as if to describe it as a little 'organic stir' was right, especially as this is the sort of thing one has when one is stirred or affected."

F, 25 cm. Coil = 2600. "Sensation is more of the strainy kind than anything else in the world." F again insists on the affective character of these sensations and describes them as "excitement" or a "stir up."

G, 40 cm. Coil = 10250. "Muscular sensation over very small area; intensity rather weak; meant a twitch."

G, 25 cm. Coil = 3750. "Quality is little different. Pain component was present; darting. Whole thing seems to have finer texture, so to speak. Distinctly a little throb, with a lancing, darting pain." G implies later that muscular sensations are involved in the throb.

It is clear that electrical stimulation does not bring out any new qualities of sensation. On the other hand, the results support our conclusion that the esophagus is sensible to

pressure throughout its length; for these complexes are all formed of pressure and pain.[35]

4. *Chemical Stimulation*

All the experiments with chemicals were performed solely on observer B.

Alcohol. Five cc. were administered in each trial with tube 2. The esophagus appears to be sensitive to stimulation by alcohol throughout its length. Ten *per cent.* alcohol brought out sensations at 35, 25, and 20 cm., and heat at 30 cm. Cold was also felt in the throat at all these places and also at 40 cm. Warmth, however, did not appear at 40 cm. Thirty *per cent.* alcohol gives warmth combined with stinging sensations at 40 cm. Slight warmth was felt in the upper parts of the esophagus for a 4% solution; and even a 2% solution gave very slight warmth at 30 cm.

The localization of typical sensations for different intensities of alcohol is shown in Figg. 96-101. Fig. 99 shows the tendency for the sensation to spread. The final stage in Fig. 99 doubtless occurs when the alcohol reaches the stomach.

The character of the experience dependent upon stimulation by alcohol may be shown as follows.

B, 30 cm. 30% alcohol. "At first coolness. Then warmth develops and gets very slightly hot. It spreads as an intense warmth with a heat component. It is well below the surface of the body and uniform over the whole area. It shrinks from the chest up toward the throat until there is just a large general warmth, left in the throat."

B, 30 cm. 10% alcohol. "Decided cold in throat at first. Then a momentary heat in throat together with a little catch of heat opposite the nipples. Then nothing; and then a burning glow in throat, *i. e.* a diffuse, indefinitely localized heat. The heat becomes intense, so that the whole upper part of the middle of the body seems to glow with it. It seems then to shrink from below up and is soon confined to the throat only. Next it gets gradually weaker, and disappears at the time that a very strong, stinging heat develops over a large area in the stomach region. This heat dies down quickly until there remain only weak intermittent glows and stings."

Hydrochloric Acid. Five cc. of 5% HCl were introduced through tube 2 into the esophagus at 40, 30, and 20 cm. from the teeth. The esophagus responded in all cases with an ache-like or a stinging pain, generally very faint. These pains were localized either in the throat or somewhere below the level of the nipples. Portions of the reports follow.

B, 40 cm. 5% HCl. "Very dull ache of the tooth-ache kind, back

[35] The descriptions are not unlike those of Becher's Herr W., although our observers do not report a qualitative difference between the two ends of the esophagus. Becher says: "In der ganzen Speiseröhre ergeben sich sehr deutliche Sensationen. Doch ist der Eindruck nicht überall der gleiche. . . . Im oberen Teile der Speiseröhre hat Herr W. ganz deutlich den Eindruck des Elektrisierens, also jenes charakteristischen Schwirrens. . . . Im unteren Teile bleibt nur noch eine schwach brennende oder ätzende Wirkung, die Herr W. mit der eines scharfen Salzes, eines beissenden Gesmacksreizes vergleicht." (*Zeitschrift*, 49, 352.)

of the nose; then a stinging ache, *i. e.*, a raw feeling, in the throat. This ache goes quickly; then there is a much stronger ache, which is also 'stingy,'—a sort of burn without warmth. It becomes quite intense and dies away very slowly, fusing with general ache in stomach region. This latter ache gets more intense and presently without qualitative change turns into hunger."

B, 30 cm. 5% HCl. "Intense cold in throat. Then a stinging feeling above stomach, which was more like pain than a dull ache. Shortly afterward a dull ache appeared in the same place and also lower down, and turned presently into hunger pains."

B, 20 cm. 5% HCl. "Cold at first, then stinging pain from nipples to sternum. Then intense aching pains in stomach, rather sharp, bright, and diffuse. The ache later gets quite acute, like some of the pains in the call to defecation. Still later there is nausea, which involves both this ache and certain muscular sensations."

Mustard. In this series, 25 cc. of a 20% mixture of English mustard and water were injected into the esophagus. Aside from sensations of coolness and warmth, localized in the throat, very faint, and of very short duration, the esophagus does not seem to be sensitive to mustard; although characteristic experiences involving nausea, general aches, and specific burning aches in the throat, occur approximately at the time that the mustard reaches the stomach.

Pepper. In this series 25 cc. of powdered black pepper in suspension in water (1:100) were used. In each case a coolness is felt at once in the throat; later stinging sensations are also felt in the throat. The stinging sensations are the more delayed, the farther up in the esophagus the pepper is applied. Consequently we may assume that they come from stimulation of the stomach.

Peppermint. A 2% dilution of oil of peppermint in olive oil was used. Five cc. were injected. The introspections indicate that the esophagus does not respond to oil of peppermint. The warm sensations occur after the oil has had time to reach the stomach. Olive oil alone brought out no response.

(Numbers in brackets indicate the number of seconds elapsed after the administration of the stimulus.)

B, 40 cm. 2% oil of peppermint. "[16] Cold around the sternum; then a slight burning sensation in the throat, which soon disappears. [33] Cold around sternum gradually extends on up with slight ache."

B, 30 cm. 2% oil of peppermint. "Cold in throat at once. [23] General stinging cold; turns gradually into a warm glow all along median line. [49] Glow gets very intense, a diffuse heat. No more cold. The glow is intense all the way from top of larynx to just below sternum. It dies away in throat last, and as it dies away a cold reappears."

B, 20 cm. 5% oil of peppermint. "At first, very cold in throat. [35] Faint ache in stomach. [37] Burning heat in throat begins, and gets quite intense, but does not spread below throat; lasts a long time. [230] Heat is now dying out. Some cold has returned."

5. *Localization*

It has already been shown that esophageal sensations are variously localized by different individuals and, under different conditions, by the same individual. In general, the

reference is downward for most of the sensations from the lower two-thirds of the esophagus, and the points of reference tend to group themselves, so that the error of localization is greater for higher points of stimulation. The upper third of the esophagus, however, is apt to give sensations localized above the point of arousal. Stimulation of the lower end of the tube often results in sensations in the throat, concomitant with those higher up, and either of the same or of a different quality. In the series that we are about to describe, an attempt was made to measure the errors of localization. In all cases so far mentioned, the data have been obtained by the use of a stimulus tube with but a single place upon it for stimulation; the tube had to be moved up and down the esophagus. It was impossible to keep the observer in ignorance of the position of the tube. Even had he obtained no cues from the serial method followed, or from the length of tube outside of the mouth, he might still have obtained them from the ache-like sensations aroused by the end of the tube. For these reasons tube 8 was so constructed that it could be left in the esophagus and stomach, and electric shocks could be given at various points without altering its position.

It first appeared that the esophagus was unequally sensitive at different portions of its length; so that, when equal intensities of shock were used, an intensive cue to the point of excitation was afforded. B, the only observer, knew that the lower end of the esophagus was in general less sensitive than the upper end, and was therefore inclined to look for the weaker shock in the region of the sternum. For this reason, equivalent intensities for the different parts of the esophagus were determined in the manner described (pp. 31f.). We had planned to include a point of stimulation in the stomach, and had therefore attached the sixth pair of terminals at the end of the tube. There was, however, so marked a difference in the contact made in the stomach at various times (due, probably, both to the chance position of the end of the tube and to the nature and amount of the stomachic contents), that it was impossible to get the same intensity of sensation from the same intensity of current. Since these high intensive differences were further complicated by variation in quality, we decided to exclude the stomach from this series of trials.

Twenty-five series were arranged, in each of which each of the five points in the esophagus occurred once in haphazard order, determined by drawing numbers from a box. The observer lay supine, blindfolded, and with arms stretched at his sides. Localization was made by bringing the right hand up to the body after the shock had been felt, and indicating the point with the forefinger. The observer was allowed to move the hand after he had touched the body. In a preliminary series it was found that there were a certain number of very indefinite sensations, which the observer experienced great difficulty in localizing. Hence, in the regular series, he was allowed to ask for the repetition of the stimulus when he felt very

uncertain as to its position. Sensations that seemed to cover an area, or to stretch out in length, were localized at the point which appeared to the observer as the middle.

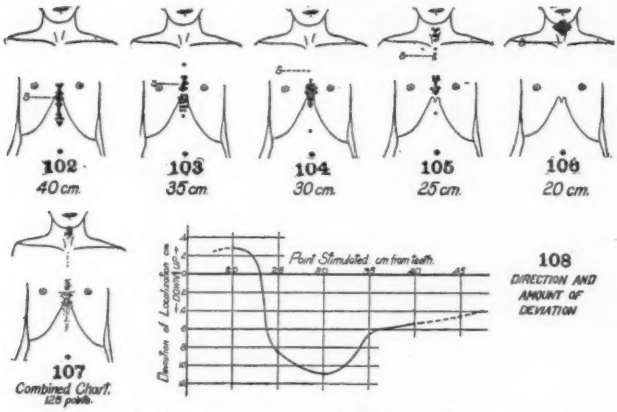
TABLE VIII

Localization of electrical stimulation of five different points in the esophagus. All figures, including the column headings, which indicate the point of stimulation, represent cm. along the alimentary tract from the teeth. The values are rearranged to show the distribution.

Point Stimulated	40	35	30	25	20
	48.5	46.	49.	45.	24.
	48.	43.5	42.	38.	16.
	48.	43.5	41.	37.	15.5
	48.	42.	41.	37.	15.5
	47.	42.	40.5	36.	15.
	47.	42.	40.	36.	15.
	46.	42.	39.5	36.	15.
	46.	41.	39.5	35.	14.5
	45.5	41.	39.	34.5	14.5
	44.5	40.	38.5	33.5	14.5
	43.	40.	37.5	33.	14.5
	42.5	40.	37.5	33.	14.
	42.5	39.	37.	32.5	14.
	42.5	39.	37.	32.	14.
	41.	37.	37.	32.	14.
	41.	36.5	37.	26.	14.
	40.	36.5	36.5	24.	13.5
	40.	36.	36.5	22.	13.5
	39.5	35.	36.	18.5	13.5
	38.5	34.5	36.	17.	13.5
	38.5	33.	36.	17.	13.
	38.	32.5	34.5	16.5	13.
	36.5	32.	34.5	15.5	12.5
	36.	32.	34.	15.	12.
	36.	27.	33.	15.	12.
Average	42.52	38.12	38.00	30.68	14.42
Av. error	2.52	3.12	8.00	5.68	5.68
M. V.	3.38	3.77	2.40	7.60	1.27
Av. error				10.33 6.35	
M. V.				2.32 3.19	

The localizations are shown in Table VIII, in which the values are rearranged so that their distribution is evident. The M. V. for 25 cm. is very large. Its size is due to the fact that the points fall into two groups, the one considerably above the point of stimulation, the other very much below. If the points are arbitrarily separated into these two groups,

so that all above 32 are in the one and all below 26 in the other (there are none between 26 and 32), the M. V.'s are greatly reduced. The figures for this separation are shown below the last line in the column for 25 cm.



FIGG. 102-108.—LOCALIZATION OF ELECTRICAL STIMULI IN THE ESOPHAGUS

102-106. Points of reference for 25 stimuli at each of 5 positions, *viz.*, 40, 35, 30, 25, and 20 cm. from the teeth. 107. Combination of 5 preceding diagrams. Note reference away from upper chest. Points show longitudinal, not transverse, reference. S = level of stimulus. 108. Curve, showing amount and direction of reference.

Diagrams for each of the five points of stimulation are shown in Figg. 102-106. The position of the points in these figures is correct longitudinally, but not transversely, since almost all were localized on the median line (only one point was localized more than 1 cm. to one side). Fig. 107 is a combination of the five figures preceding, and shows the longitudinal position of all the 125 points. Fig. 108 shows these amounts of deviation (*i. e.*, the average errors) platted in the form of a curve. The curve is steep below 25 because there are few localizations in this region. The dotted lines indicate a probable extension of the curve.

The result is what we had expected from the rough localizations of the preceding experiments. Reference is away from the upper portion of the chest. This fact was recognized, at least by B and F, in their introspections. B constantly

complained that inside himself he "had no neck;" that is to say, he felt sensations in his throat and in the region of the sternum, and believed them to be part of the same complex, although they did not seem to be localized in the intermediate region. B explained the continuity by saying that the sensations were continuous when visualized from within, but they were discontinuous when visualized as underlying the various parts of the body. The difference was very striking: when he visualized from the outside, there was a constant fluctuation of the visual image (more often an alternation than a co-existence); but, when he visualized from within, not only was there but a single sensation to be found, but there seemed to be nothing else to which he could attend. Apparently the spatial pattern within the chest does not correspond with that of the outside.

On the basis of these results we cannot agree with the statements of Becher. Concerning cold and warmth sensations in the esophagus he writes: "Die Lokalisation— in der Höhendimension und nach einer obigen Bemerkung über die Feststellung der unteren Linksabweichung auch in der Breitendimension—ist innerhalb gewisser Grenzen zuverlässig."[36] "Herr W. empfindet also zweifellos in der ganzen Speiseröhre Druck- und Berührungsreize. Diese werden ziemlich gut lokalisiert."[37] For electrical stimuli, "das Lokalisieren ist ziemlich gut, wie bei den Druckreizen."[38] Hertz is even more positive and states "that the whole esophagus is sensitive to thermal stimuli, and that the power of localization is extremely accurate, particularly in the case of heat."[39]

The tendency to refer the sensations which originate in the esophagus in the region of chest to the soft parts of the trunk may throw some light upon the means by which ability to localize is acquired. Our experiment requires the observer to localize sensations from the esophagus upon the surface of the body. The only parts of the body-wall on which a stimulus, affecting the surface of the body, would also affect the esophagus, are the soft parts below the sternum and above the clavicle. These parts may thus have become associated in experience to the corresponding parts of the esophagus. The probability that stimuli below the sternum would affect the esophagus farther up under the hard chest-wall than the stimuli at the neck would affect it downward and underneath may explain the facts (1) that sensations underneath the chest-wall are referred a greater distance downward toward the sternum than they are upward toward the neck, and (2) that the part of the esophagus below the upper chest is

[36] *Zeitschrift*, 49, 352.
[37] P. 351.
[38] P. 352.
[39] *Op. cit.*, 6.

the seat of the most indefinite sensations, those which are the hardest to localize. If the observer could in some way reach down his esophagus and touch the actual point stimulated, as he may do in localizations upon the skin, it seems probable that the accuracy of localization would be found to be much greater.[40]

III. The Stomach

1. *Thermal Stimulation*

Sensibility. The stomach was stimulated with 25 cc. of water at various temperatures. Subsequently a special series with varying temperature was taken with the aim of determining whether the stomach is sensitive to cold or warmth, and, if so, at what temperatures. In this series 50 cc. of water were used. The combined results of all observations are given in Table IX. The grouping of observations made

TABLE IX

Sensibility of the stomach to temperature. Delayed sensations (probably due to the spread of the stimulus) are placed in parenthesis.

Stimulus Temp., °C.	Observers			
	B	D	F	G
0	cold	cool	cold	cold
10	cold	no temp.	no temp.	cold
19	cold			
20	cold	no temp.	no temp.	cold
23	cool			
27	cool			
30	cool	no temp.	no temp.	cold
31	no temp.			(cool)
25			no temp.	(cool)
39				(cool)
40	no temp.	no temp.	no temp.	
47	no temp.			(warm)
50	heat	no temp.	no temp.	warm
51				(warm)
55	heat			
60	warm	(warm)	no temp.	heat
70	heat	(warm)	warm	warm-heat-pain?
80	heat-pain			

[40] Meumann suggests that the esophagus might become educated in localization by the successive sensations occurring in the swallowing of food (*Archiv*, 14, 288f.). He does not seem to realize that the kind of localization under consideration requires that there should be an association between the internal sensations and processes representing the exterior. Becher has criticized his position (*Archiv*, 15, 358f.).

with different amounts of water and in different orders is justified by the fact that the stomachic conditions are never constant (from variation of stomachic contents), and also by the remarkable consistency of the data. Only those sensations that followed immediately upon stimulation are recorded, save in cases when no temperature sensations occurred until late. The quality of the delayed temperature sensations is entered in parenthesis.

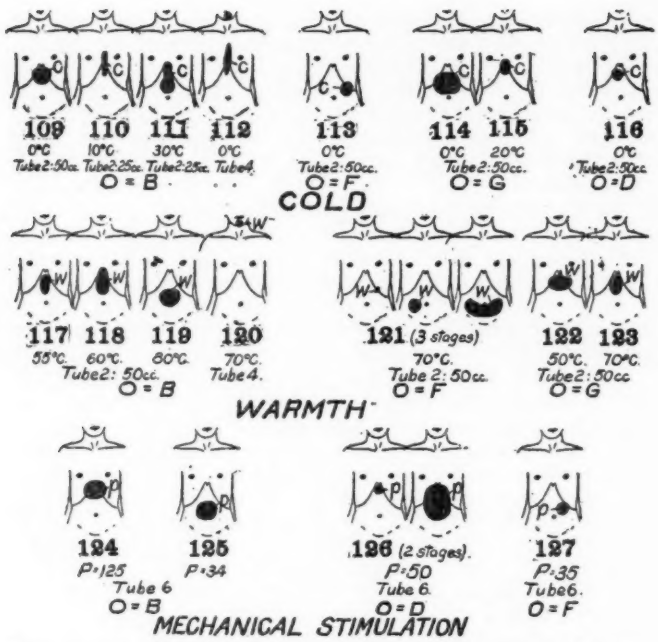
All the observers are sensitive to cold at 0° C. B feels cold at 20° and coolness at 30°. G feels cold at 30°. The delayed coolness that G finds for temperature as high as 39° may be due to conduction to the body-wall.

If we consider the delayed cool of G as not aroused in the stomach, then for all the observers there is a *Nullpunkts-temperatur.*

B once reports heat at 50°, although the result could not be duplicated. He always finds heat or warmth at 55° or 60°. G reports heat at 60° and warmth and heat at 70°. F finds warmth at 70°. D does not find either heat or warmth, except after some delay, for 70° or less. The experimenter hesitated to go above 70°, except in his own case, for fear of injuring the tissues. B finds thermal pain at 80°, and G suspects it but is not sure of it at 70°. We may say in general that B, F, and G appear to be sensible to warm stimuli, and that D shows no such sensibility within the limits of the experiment.

The sensibility of the stomach to cold and warmth was also demonstrated in B by the use of the water coil (tube 4) at 0° C and at 60° C. The electric heating coil (tube 3) did not give definite warmth at all.

Localization. The localization of cold from the stomach is shown in Figg. 109-116, and the localization of warmth or heat in Figg. 117-123. The points to which cold is referred seem approximately correct, though they are on the average too low. The localization to one side in Fig. 113 may be due to the actual position of the tube in the stomach, although F showed an individually marked tendency to refer incidental internal sensations to this side. Figg. 119 and 121 indicate the spread of the warmth. In Fig. 121 it is not unnatural that, in spreading, the warmth should no longer remain at the side, although it is surprising that it should lie entirely below the umbilicus. The only sensations referred to the throat were those of B (Figg. 112 and 120), in which the water coil (tube 4) was substituted for tube 2.



FIGG. 109-127.—LOCALIZATION OF THERMAL AND MECHANICAL STIMULI IN THE STOMACH

O = Observer; P = maximum intensity of pressure (cm. of water). c = cold; p = pressure; w = warmth.

Quality of Sensation. There can be no doubt that the cold experienced upon stimulation of the stomach is qualitatively like cutaneous cold. It is sometimes more peculiarly sharp and intense than the ordinary cold felt on the skin, and more like that from the glans penis. B describes the cold as "quite intense, sharp, very bright and pleasant, quite different, like paradoxical cold." D mentions only a "slight coolness." F reports "cold sensations, weak in intensity, overlying and in front of the bubbly sensations." G speaks of "sensations of cold, very clear and fairly intense," and later of a "more intense cold, low and narrow, combined with a weaker cold near waist line." She adds that she could not tell exactly where the boundaries of the areas were.

In order to get a comparison of the qualities of cold from the stomach and from the skin, the following experiment was arranged. The stomach tube was taken into the stomach, and a hot-water bottle wrapped in a cloth was placed on the abdomen and connected with another tube. Water was pumped into the bottle and into the stomach at the same time. At one time, cold water was pumped into the stomach, and water at body temperature into the hot-water bottle. At another time, the neutral water was given inside and the cold water outside. The observer described his experience. In the first trial, with the cold outside, B, the only observer, thought first that the cold came from the stomach; but later, when it had spread over a larger region than was usual for stomachic colds, he identified it as originating in the skin. When the temperatures were reversed, he was at first uncertain as to the origin of the cold, but later identified it as internal, because it was duller, heavier and less bright: adjectives which, he says, indicate a temporal, spatial, and intensive, but not a qualitative difference.

In a second trial cold water was injected simultaneously into the stomach and the hot-water bottle. B recognized the internal cold as coming first because it was accompanied by the typical reference of cold to the throat. He also believed that the external cold lasted longest because of the large area affected at the end of the trial and because of the pressure cues from the bottle. He was totally unable, however, to say at what point the internal cold passed over into the external or whether they both existed together at any time, and declared that the two experiences were identical in quality.

We may question the origin of the sensations of cold in internal stimulation in the stomach; but there can be no question that, wherever they originate, they are in quality like the sensations from the skin.

The sensations from hot stimuli are described both as heat and as warmth. B, for example, reports: "Heat suddenly begins and spreads in most alarming fashion; gets more intense and spreads rapidly (see Fig. 119). Almost felt as if whole front of my body were hot; like a flat sheet of heat a little below the surface. Very 'stingy,' bright and piercing, but not truly painful." Again he reports: "Besides the pressure, there is warmth from sternum down; then a general heaty warmth in the stomach region, almost burny, not so unlike pepper. After the trial, there is a general warm glow for some time." D, after an 8 sec. interval, reports a "warmth;" after 15 seconds, "different warmth;" after 32 seconds, "almost hot;" after 43 seconds, "still warm;" and after 78 seconds, "very indefinite, becoming a general glow." F, who had felt no warmth at all below 70°, found at this temperature a "little spot of warmth, quite warm, as small as the end of my finger," which "quite slowly spread over a large area. The more intense part of it forms a core about twice the size of a dollar." G states that there are "warm sensations to begin with, which become quite hot. They are located in the stomach; the area is longer than wide. They

become diffuse as time goes on, and are located more and more toward outside of body."

The experiment with the hot-water bottle described above was repeated with water at 37° and at 60°. The observer (B) was not deceived as to the location of the warmth, because on the outside the warmths were felt to agree spatially with the sensations of pressure from the bag, and on the inside, heat sensations were felt in the throat. B suspected, however, a qualitative difference between the two; the internal warmth seemed duller, more diffuse, and had something of the character of an ache in it. When warm water was injected both inside and out the internal sensations were interpreted as preceding the external, principally, as was the case with cold, because the sensations referred to the throat were felt first and the pressure of the hot-water bottle last. In the middle of the course the observer could not distinguish between the two complexes nor state where one began or the other left off. All the warmth was localized in the same region about the same distance beneath the surface of the body. The early warmths were more heat-like and more diffuse than the later ones. The presence of heat or even of an ache does not, however, argue for a qualitative difference, but merely for a greater effectiveness of the stimulus in the one case. Diffuseness may be spatial, dullness intensive. It is not conclusively shown that there is any qualitative difference at all.

Seat of Sensation. There can be little doubt that many of the delayed sensations following thermal stimulation of the stomach originate in the nervous organs situated elsewhere, perhaps in the skin or the body-wall. Often a warmth or a coolness will occur some time after the stimulation, and will spread well over the body, the whole temporal course occupying a minute or more. Whether the immediate sensations can be attributed to the stomach is not entirely obvious, especially as there is no qualitative difference between internal and external cold, and as the apparent difference in the case of warmth may be due to the fusion of an internal 'ache' with an external warmth. The extreme temperatures from 50° to over 70°, which are necessary to bring out the warm sensations immediately, may mean that no warmth can be felt unless the conditions are such as to promote rapid conduction. In favor of the position that temperature sensations depend upon conduction away from the stomach to more superficial tissues is the fact that their delay is decreased by increasing the temperature from 50° to 80°. Finally the qualitative similarity—not to say identity—of the experiences dependent upon external and internal thermal stimulation is evidence for their common seat. The case is perhaps still unproven, but the writer inclines to the belief that the actual tissues of the stomach do not mediate the sensations of cold or warmth,

but that these sensations arise upon internal thermal stimulation by conduction to the body-wall.[41]

2. *Mechanical Stimulation*

Sensibility. In all observations both pressure and pain sensations were elicited by inflation of the bladder of tube 6. The intensities, expressed in cm. of water required to produce a just noticeable pressure and a just noticeable pain, are as follows:

	B	D	F	G
Pressure.	12	10	20	25
Pain.	35	50	35	35

It is again difficult to state whether these sensations originate in the stomach or whether they come from surrounding tissues as the result of distension of the stomach. In extreme inflation, the pressure spreads down into the abdomen, and when the air is released there is a general feeling of relaxation. These sensations undoubtedly come from the body-wall, and possibly from other organs, and there is no assurance that the lighter pressures are not also mediated by the surrounding tissues.[42]

Localization. The localization of pressure in the stomach is shown in Figg. 124-127. The reference is usually to a point below the stomach, sometimes to one as low as the umbilicus. This fact is in accordance with the belief of most people that the stomach is well down below the ribs, as well as with the recent anatomical conclusions.

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[41] This conclusion is in accord with those of Becher (*Zeitschrift*, 49, 350), Hertz, Cook, and Schlesinger (*op. cit.*, 483), and of Hertz (*op. cit.*, 7), although the last writer admits that the stomach may "rarely" be sensitive to heat and cold (p. 9). The present writer plans a further detailed study of this point for the future.

[42] Hertz states that the stomach is insensitive to tactile stimulation (*op. cit.*, 3), but that the sensation of 'fulness' is experienced at a pressure of about 18 cm. of water (p. 19), a value that accords closely with our own. Becher, however, insists that the stomach is insensitive to pressure and that the sensation of fulness is due to expansion of the body-wall (*Zeitschrift*, 49, 356f.). Meumann speaks of "eine charakteristische Empfindung von Fülle und Druck im Magen" (*Archiv*, 9, 52f.), but probably does not mean typical pressure. He is interested chiefly in the *Verdauungsgefühle* and takes pains to distinguish the sensations of *Fülle* and *Leere* from the *Druck- und Zugempfindungen* of the skin of the abdomen and also to separate the feelings of *Fülle* and of *Sättigung* from each other (*Archiv*, 14, 292, 295). He ascribes hunger partially to the stomach (*Archiv*, 9, 152).

Quality of Sensation. The following reports were made upon the quality of the stomachic sensations.

B, Max. press. = 25 cm. "Dull ache, fairly well down; distinct from the mere pressure sensations above. The ache is sharply painful, but is something like a sensation from an object stuck in the throat. It seemed perfectly simple and elementary, and like pressure plus pain. It is something like hunger, but more diffuse and cloudy. The boundaries are indefinite."

B, Max. press. = 34 cm. "At first I begin to feel pressure. General distension got gradually more intense and very definite; similar to sensation of fullness after a big meal and somewhat like cutaneous pressure, *i. e.*, not at all 'achy.' Quite definite. Perhaps it is cutaneous pressure; my body-wall feels bulged out. Later, I feel as if I were going to burst. I think the sensations on release of pressure are entirely cutaneous."

D describes the stomach pressure as "large" and "bloaty."

F, Max. press. = 30 cm. "Very weak muscular-pressure-like sensation. Then I got a pole of pressure just a little to the left and above umbilicus, moderate intensity. Pole increases in intensity and gets larger. Finally gets painful. Pressure very intense. Pain more scary than intense. Made me break into sweat. Strain sensations came in somewhere quite superficially, way up near the sternum; a bit colicky and more superficial than the strain sensations accompanying the pain above. Could feel the outside body-surface expand; probably tactual sensations."

G gets slightly painful sensations of "crampy nature, neither stabbing nor sore," finally however becoming "definitely crampy and muscular."

It is evident we have found no new quality of sensation by mechanical stimulation of the stomach. The pressures and dull pains are of the same order as those felt in the esophagus and, to a certain extent, below the surface in any portion of the body.

3. *Electrical Stimulation*

Sensibility. The stomach proved to be variously sensitive to electric shock with tube 7. The minimal stimulation occurred within the following intensities (Kronecker units); the variation was due to chance differences of contact in the stomach or of the short-circuiting effect of the acid in the stomach.[43]

B	D	F	G
3750-7600	6250-10250	3750-4950	4950-10250

Localization. The localization of these sensations is best shown in the results of a special series upon B. Tube 7 was used and the coil was set at 4950 units. Stimulations were given with the tube at 45, 50, 55, and 60 cm. from the teeth,

[43] Becher reports that the stomach is insensitive to electrical stimulation (*Zeitschrift*, 49, 356).

five trials in each position, twenty in all. The tube was pulled up and forced down each time and the positions were selected in a random order. The results, which show a degree of uniformity greater than that for the esophagus, follow.[44]

Position of tube.....	45	50	55	60
Average localization	38.2	42.8L3.6	50.2L3.6	51.8R0.6
Mean variation	1.0	1.3L1.1	1.0L0.5	1.8R2.5

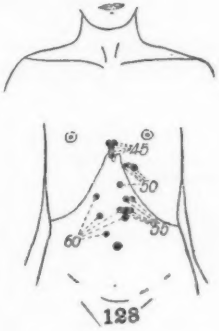

FIG. 128.—LOCALIZATION OF ELECTRICAL STIMULI IN THE STOMACH

Points of reference for 5 stimuli for each of 4 lengths of tube within the stomach, *viz.*, 45, 50, 55, and 60 cm. from the teeth.

Fig. 128 shows the localizations diagrammatically. The exact error of localization can not be determined because the exact position of stimulus is indeterminable. Neither can the position of the stomach be stated with certainty,[45] nor can the course of the tube within it be known. The localizations, however, for a given length of tube group themselves quite closely, those for the longest lengths lie farthest from the cardiac region, and the course of variation—first downward and then to the right—is that which might be expected in the most usual stomach.

[44] Meumann has already maintained that with palpation one may come to localize the stomachic sensations very definitely (*Archiv*, 9, 52).

[45] "There is no organ in the body the position and connections of which present such frequent alterations as the stomach" (Gray, H., Anatomy, Descriptive and Applied (Spitzka), 1913, 1005). Recent radiographic work has shown that the stomach may frequently extend as low as the umbilicus or even entirely below it, and that both position and size vary very greatly with the individual and with internal conditions in a single individual. *Cf., e. g.*, the radiographs reproduced by v. Elischer, J., Ueber eine Methode zur Röntgenuntersuchung des Magens, Fortschr. a. d. Geb. d. Röntgenstrahlen, 18, 1912, 333ff. No wonder that Herr B. in Becher's experiment appeared to localize "sogar fast unterhalb des Magens."

Quality of Sensation. The observers had great difficulty in describing the quality of intense induction shocks. The weak ones were, however, described as faint catches or jerks, and the observers all inclined to the belief that there was no difference from the sensations that would be felt under similar conditions on the periphery. The shocks were much brighter and less ache-like than those in the esophagus.

It may seem that the production of kinesthesis-like sensations in the stomach by weak electrical shocks argues for the sensibility of the stomach itself. The proof, however, is not positive; for the more intense shocks sometimes made the body wall twitch; and no shock greater than 3750 units can well be considered as so weak that it would be unlikely to spread (see p. 32).

4. *Chemical Stimulation*

Alcohol. Various concentrations of alcohol were blown into the stomach with tube 2, five cc. at a time. Faint warmth was felt for 60% and also for 100%. With the latter, coolness was felt below the sternum at first, and then a very faint warmth, which is a little like heat, that is to say, a faint burning which shifts intermittently over a fairly large area. Later, there is "an uncomfortable feeling," which consists of a dull ache and a pressure, such as one has when sick at the stomach, only much fainter.

Hydrochloric Acid. Both 5% and 10% HCl produce, in the stomach, sensations very much like those constituting the ache of hunger. B reports for 5% "hunger, or a strong intense or diffuse ache, getting fainter."

Twenty *per cent.* HCl produces dizziness in the head, in the region in which the "ice-cream pains" are localized, warmth in the face, a feeling of tenseness in the region of the stomach, a vague ache in the same region, and tingling, 'achy,' 'nervous' sensations in the arms.[46] Later the ache in the stomach changes, and is recognized as nausea, although its similarity to hunger is also noted. In fact, the observer thinks that the difference between hunger and nausea is not qualitative.[47]

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[46] Hertz, Cook, and Schlesinger do not find the stomach sensitive to HCl (*op. cit.*, 486); but Becher mentions several experimenters who obtained sensory responses with fairly strong solutions (*Archiv*, 15, 365f.).

[47] The writer has been at some pains to gather introspections upon hunger, nausea, thirst, and other organic experiences. He reserves their publication until such time as he shall be able to prepare a more thorough introspective study of organic sensations. The adequacy of HCl as a stimulus to hunger suggests that it must set up the hunger contractions (*cf.* Cannon and Washburn, *op. cit.*, and Carlson, *opp. citt.*). The present writer found that HCl in the intestine produced contractions and the call to defecation. Cannon, however, says that it produces normal peristalsis in the stomach and stops hunger.

Mustard. B swallowed six capsules, each containing one-half gram of mustard, at intervals of two minutes. At first, there were aches in the upper part of the esophagus, very dull and pressure-like. Later came pains and swimming sensations in the head, referred principally to the eyes and to the back of the head. Next the ache in the esophagus became intensely warm and assumed a very uncomfortable, insistent, burning character. It was shot through by a core of sharp stinging burns localized in the throat. Next came muscular sensations in the throat, "as if the esophagus were stopped up." Later the dull 'achy' pains spread to the arms, the heart, and other deep parts of the body.

Pepper. B swallowed successively at two minute intervals six capsules, each containing one-third of a gram of black powdered pepper. This experiment resulted in warm sensations in the throat above the clavicle. The sensations were faint, but very 'stingy.' Later the warm got more intense, and was accompanied by an 'achy' pain, also referred to the throat. Much later, it became hot. The heat and pain extended all the way from the throat down to the region of the stomach, remaining however much more intense above. The heat complex, which was very much like a cutaneous heat, including the usual 'sting' and occasional brighter 'shots' of pain, lasted a longer time. Even half an hour later, when it had become intermittent, heat returned frequently upon swallowing. Still later, over an hour after swallowing the pepper, warmth continued to be felt intermittently in the throat.[48]

Peppermint. One-half cc. of 10% oil of peppermint in olive oil was swallowed in a capsule by B. At first cool sensations in the throat and the upper esophagus were felt. Then there was a very vague warmth accompanied by the typical esophageal ache. Still later the whole throat began to burn with a complex of warmth, 'sting,' ache, and pressure, very well fused, and with cold, not so well fused with the others. The burning feeling is the characteristic feeling of rawness, a sort of 'cold heat.' With it there is a 'drawing' sensation in the region in which 'ice-cream pains' are felt in the head.

IV. The Rectum

The experimental work upon the rectum and intestine was performed upon B only.

1. *Thermal Stimulation*

The sensitivity of the rectum to temperature was studied by means of tube 9 and the apparatus of Fig. 1. Tube 4 was also used in the rectum. The points selected for stimulation were those 5, 10, and 15 cm. respectively from the anus. In being pushed in even as far as 15 cm. the tubes sometimes pressed against the intestinal wall, causing considerable discomfort. At first the experimenter attempted to work at

[48] Meumann's description (*Archiv*, 14, 293f.) does not indicate that the sensations were referred to any region other than that of the stomach.

20 cm., but the difficulty of forcing in a heavy and not too flexible tube led him to give up the attempt. At one time 30 cm. of tube 9 were inserted, with the result that the end of the tube pressed against and distended the body-wall at 62 L 10 (Plate II). The distension was somewhat painful and alarmed the observer, so that, although observations were taken at this point, no attempt was made to repeat them.

The following extracts show the results of the use of tube 9.

0° C. 15 cm. "A slight coolness which spread a little to both sides was felt at the anus. It was very faint and soon disappeared."

0° C. 10 cm. "A thrill of cold that I can hardly localize. I visualized it as several silver threads running across below the surface of the nates. Cold was very intense, but small in area; always keen; lasted a very short time."

0° C. 10 cm. "Several tiny thrills of cold, very difficult to localize; I referred them to the glans and other parts of the penis. Later, after an effort to determine their position, they seemed to lie out in the air away from that part of the body. It seems as if the reason that I can not place them on the body is that, although they seem to be in certain places, the attention to these places (*i. e.*, to the other sensations referred to these places) seems to give a tactual group without the cold sensations, which are somehow different from the tactual."

0° C. 5 cm. B finds "several quite intense, little thrills of cold," which he localizes in the left inguinal region and in the left thigh. He observes again that the tactual sensations which accompany attention to these parts, as well as those that occur when he touches these places with his hand, seem to be peculiarly separate from the cold sensations and unconnected with them. He is inclined to believe that this lack of connection is nothing else than the failure of the two groups to become clear at the same time.

With the water coil of tube 4, B finds, for all positions, a coldness in the region of the anus, which does not occur until the walls of the tube begin to become slightly cool. At 15 cm. he reported a very faint sensation, which he thought at first was going to turn either into cold or into heat. He was not, at any time, able to identify it with confidence, but thought that it might be a weak form of the 'sting' of heat. The same experience occurred at 10 cm. At 5 cm. a slight coolness above the anus was felt.

It appears probable, then, that the rectum is sensitive to cold only in the region of the anus. Cold water, injected into it, gives very intense, bright, cold sensations, which are referred to various places in the region of the perineum. Cold stimulation of the upper rectum produces an indefinite sensation that belongs probably to the pain family, possibly a form of 'thermal pain.'

For warm stimulation 50 cc. of water at 50° C. and at 70° C. were injected through tube 9. It was found that warmth, unlike cold, tended to produce the call to defecation. The call was very marked at 50° C., and at 70° C. it was so violent,—involving intense, griping pains, a general bodily disturbance, and a very strong impulse toward

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a motor response,—that it interfered with the introspection. The amount of water in this series was accordingly reduced to 25 cc. The call still remained quite definite, but the pains decreased very much.

At 5 cm. and 50° C., B felt a warmth "quite intense although not heat." It was localized deep in the body, well toward the back and about two centimeters above the coccyx. At the same time, with 25 cc. at 70° B reported a heat, which fused with the muscular sensations involved in the call of defecation, and which turned later into warmth.

At 10 cm. and 50°, B was "surprised at the absence of temperature." At 70°, however, he reported that, after a period of faint pressure, there developed "a sudden burning heat, with pain, followed at once by the call to defecation." Twelve seconds later the heat became warmth, and then gradually faded out during the minute following.

At 15 cm. and 50°, B found no positive temperature, although he remarked that it was very difficult to distinguish between very faint warmth and light pressure. At 70°, warmth was felt in the region of the anus.

In the exceptional case, mentioned above, in which the tube was pushed in to 30 cm. and forced against the body-wall at that point, no temperature sensation was felt for water at 50° C. at the place where the tube ended, although there was general warmth at the anus. Sensations of pressure were definitely referred to the place at which the tube ended. At 70°, besides the warmth at the anus and a general warmth, around the umbilicus, heat from the end of the tube occurred in the same place as the pressures.

With the water coil (tube 4), 25 cc. of water at 50° and at 60° C. were used. A single trial at 70° proved very painful.

At 5 cm. and 50° there was only a very faint sensation which B thought might be either pressure or warmth. At 60° there was pain immediately, and later warmth and heat at the anus.

At 10 cm. and 50°, there was a faint warmth, somewhat delayed, at the anus. For 60° B again hesitated between warmth and pressure, but, as the sensation became more intense, decided in favor of warmth, which appeared to be localized in the scrotum and perineum until he touched those parts. Then the warm sensation seemed to recede farther in. At 15 cm. for both 50° and 60°, warmth was felt after a time at the anus. In the second case, there was also pain and the call to defecation.

We may conclude, then, since warmth is not felt until there has been sufficient time for the tube to become warm, and since in the trials with tube 9 the rectum does not respond with a sensation of warmth except very near the anus, that the lower part of the rectum only is sensitive to temperature. The sensitivity of the anus itself to both cold and warmth was evinced again and again by the fact that cold or warm water, which had been injected and which no longer produced any sensation, felt very cold or warm as it passed the anus in being expelled. The heat felt at 70° and 30 cm. from the anus does not necessarily indicate that the intestinal tract is sensitive to warmth at that place, for 50° did not produce

any temperature there, and the end of the tube was so close to the body-wall that conduction to superficial organs might have been almost immediate with a higher temperature.

The immediate response of the rectum with pain to a temperature of 60°, and the fact that this pain was of the quality of most internal pains, indicate that what rectal sensitivity to thermal stimulation exists is of a painful character.[49]

Warm water in the water-coil did not produce the call to defecation as strongly as did the warm water in direct contact with the rectum. Warmth probably makes the distension of the bowel by the water more effective as a stimulus for defecation, but is inadequate alone.

2. *Mechanical Stimulation*

Mechanical stimulation of the rectum was accomplished by the use of the bladder of tube 11. The apparatus of Fig. 2 was used. For all positions of the tube, the resulting experience was much the same. At first a very faint pressure was felt. The liminal values were approximately 30 cm. of water for the position of 5 cm. from the anus; 24 cm. of water for 10 cm. from the anus; and 20 cm. of water for 15 cm. from the anus. This pressure is described as dull and diffuse and is localized in the region of the rectum; that is to say, B has a visual image of the place, deep in from the back and above the coccyx. Later the call to defecation is produced (at 50 cm. of water for 5 cm. from the anus; at 30 cm. for 10 cm.; and at 32 cm. for 15 cm.). The call is very complex, and gets more so as the pressure is increased. It is produced less easily near the anus. It always involves pressure, colored with a dull ache, similar to the ache-like pressures of the esophagus, and localized in the rectum. These pressures fall into a pattern, which means, for B, movement of the bowels. There are also "general pressures" in the abdomen, which come when the call gets at all intense. They are localized in the region of the umbilicus, but extend well over that portion of the body. They are less ache-like than

[49] These conclusions accord with the original results of Weber (Weber, E. H., Der Tastsinn und das Gemeingefühl, Wagner's Handwörterbuch der Physiologie, iii, 562ff.) and with those of Hertz, Cook, and Schlesinger (*op. cit.*, 485). Head, Rivers, and Sherren, on the contrary, hold that the intestine is sensitive to extremes of temperature (*op. cit.*, 112), and Becher observes that intestinal cold is felt, but localized in the anterior abdominal wall (*Archiv*, 15, 375f.). Meumann insists on the general sensitivity of the intestines, but does not directly consider thermal sensibility (*Archiv*, 14, 310).

the other pressures. Besides the pressures, there are different pains which range from a dull, diffuse, and never more than moderately intense ache to a sharp keen intense pain, which may be either massive or bright (probably an extensive difference). B found it convenient to think of the organic pains as arranged in a two-dimensional manifold, in which an intensive change from 'weak' to 'strong' was paralleled by a qualitative change from 'dull' to 'bright.' Extensive differences, for example those involved in the distinction between fine and heavy, diffuse and clear-cut, occurred independently of the intensive-qualitative relation.[50]

In the call to defecation, all sorts of pains were involved,—bright, intense, fine pains referred to the penis, scrotum, perineum, and abdominal wall, as well as deep, heavier aches within the abdomen. The scrotal pains were usually slightly more massive than the tiny, wiry, shooting pains in the penis and perineum.

We cannot say whether these pressures originate in the rectum itself. Not only is the rectum distended by the rubber bladder, so that adjacent tissues are affected, but the stimulations seem to produce movement over a large portion of the colon, so that the secondary sensational response is widespread.

The pains from the rectum may come from the canal itself or from the peritoneum. The definite ache-like pains, that occurred when the tube was forced against the wall of the rectum, and that could be brought out with very great intensity by pressing the body-wall against the end of the tube at 30 cm., may have come from the intestine, but were more probably caused by the distension of the peritoneum.[51]

3. *Electrical Stimulation*

For electrical stimulation tube 12 was used with the apparatus of Fig. 3.

The rectum is more sensitive near the anus than farther in. The setting of the coil required to produce a just noticeable sensation was 1500 units for 5 cm. beyond the anus and 3750 for 15 cm. Shocks just slightly greater than these liminal values were used as stimuli.

[50] The qualitative classification of organic and cutaneous pains needs to be worked out. A great variety of stimuli adequate to different sorts of pain are indicated in this paper. The writer plans at present to utilize them for a qualitative study.

[51] Our results accord with those of Becher (*Archiv*, 15, 373f.) and Hertz (*op. cit.*, 28ff.).

In quality, the electric shock produced a sensation like muscular pressure. B identified it absolutely with muscular pressure for points well within the rectum. Just beyond the anus, however, it seemed to him to contain more of the brighter quality of cutaneous or subcutaneous pressure. Here there were also traces of an ache, and the whole experience was more or less like an electric shock on the skin. Pain occurred from stimulation well within the rectum only once, and was then described as the "bright, diffuse pain usual in electric shocks."[52]

The sensations resulting from shock were localized fairly correctly for the position just beyond the anus. When the tube was 10 cm. in, they were again felt in about the same position, *i. e.*, deep, and slightly above the coccyx. At 15 cm., however they were referred out into the flesh of the nates. The sensations seemed to extend in a long transverse line at one time across both nates, at another time upon the left natis only, and a third time in a smaller region to the left of the median line.

4. *Chemical Stimulation*

Alcohol. Ten cc. of 30% alcohol, injected with tube 10 at 15 cm. from the anus, resulted at once in stinging pains and the call to defecation, and later in a widespread bodily reverberation, including shivers in the legs and sweat in the chest. Expulsion of the alcohol was exceedingly painful, for it made the anus smart and burn.

Hydrochloric Acid. Ten cc. of 5% HCl introduced at 15 cm. from the anus gave "warm, stinging sensations, in which the sting is bright, somewhat like cutaneous prick." The sensations were localized 5 or 10 cm. within the rectum. Two *per cent.* gave no relevant sensations, although the call to defecation followed shortly.

At 10 cm. from the anus, 5% HCl produced a complex muscular pattern in addition to the warm, stinging sensation described above. Four *per cent.* gave a cool thrill at first and later the stinging sensation. In both cases the call to defecation was very strong indeed, involving pressures all over the abdomen.

A faint sting was felt preceding the call to defecation with 10 cc. of 2% HCl were injected 5 cm. beyond the anus. Expulsion of the acid was very uncomfortable since it produced "raw" sensations in the anus. These sensations were, however, not nearly so intense as those occurring in the expulsion of the alcohol.

Mustard. A mixture of water and mustard (5:1) was injected at 15, 10, and 5 cm. beyond the anus. There were no results beyond a weak call to defecation.

Pepper. Pepper suspended in water (1:100) was introduced at the three usual places. Aside from the sensations involved in the call to

[52] Becher's description of electrical stimulation within the rectum is similar to the present one. He finds dull shock sensations at 20 cm. and bright sensations at the anus (*Archiv*, 15, 374f.).

defecation, which was not intense and occurred in all cases, little thrills of intense cold were experienced. These were referred to the neighborhood of the sphincter and to the anterior surface of the body just above and to the left of the *symphysis pubis*. They occurred immediately and were probably caused by the temperature of the water, which was at 18° C.

Peppermint. Five cc. of oil of peppermint were injected at 15 and 10 cm. from the anus. In the first case, a faint coolness was felt immediately in the rectum. Later, on expulsion of the oil, there were cold burning pains at the anus. The anus felt raw and irritated for some hours afterwards. Apparently the upper rectum is not sensitive to the oil.

V. The Colon

1. *Thermal Stimulation*

An enema of about a quart of water at 50° C. was given in order to see if there were any temperature sensations above the rectum. The usual pressures and pains involved in a violent call to defecation occurred, but there were no temperature sensations referred to any place above the rectum. A second enema at 60° C. produced so much pain that it was necessary to discontinue it before any general warmth had been noted.

An enema of a pint of water at 0° C. resulted successively in cold in the anus, an ache in the penis, an ache-like pressure in the rectum, and finally an indefinite sensation in the region of the umbilicus. This 'indefinite sensation' B describes as "a faint sting, which was possibly cool. It is very much like the sting in 'cold heat,' *i. e.*, the sensations of heat that one gets from a cold stimulus at times during nerve regeneration. It is, however, very much weaker than the cold heat to which I am accustomed."

2. *Pressure*

The various pains and pressures involved in the call to defecation have already been noted in the description of sensations from the rectum (p. 53). The inflation of the whole bowel with air failed to produce anything new. B reported definitely that the pressure was always of a dull muscular variety, always diffuse and never bright. The pains were the usual aches of distension, which get brighter and sharper with increase of intensity (p. 53). A moderate amount of inflation resembled the normal feeling of fullness which ordinarily represents the need for defecation. Greater intensities resulted in typical "belly pains."

SUMMARY

The esophagus is sensitive to warm and to cold stimulation throughout its length. Mild stimuli give rise to sensations which are qualitatively like the sensations of cutaneous warmth and cold. Stimuli of approximately 60° C. give rise to heat, which probably includes a paradoxical cold. Extreme cold or heat results in pain. The temperature sensations are sometimes referred to the throat from lower regions, and the thermal pains are often localized in the head.

Mechanical stimulation, which results in the distension of the esophagus, is sensed at weak intensities as pressure and at greater intensities as pain. Pressure is more readily elicited by rapid than by slow distension. In quality the pressure is like the muscular pressure, and the pain like the deep pain, of the forearm.

The esophagus is everywhere sensitive to bipolar faradic stimulation, and increases in sensitivity toward the pharyngeal end. The experience is qualitatively similar to the pressure-pain complexes of electric shocks upon the periphery.

The esophagus is sensitive to alcohol and hydrochloric acid, but not to oil of peppermint or mustard or pepper in suspension.

The sensations from the esophagus are in general referred either to the region below the sternum or to the throat. The amount and direction of reference are fairly constant for any given position, and the amount is greatest mid-way between the clavicle and the nipples. At this point sensations are referred in either direction, or sometimes in both. Transverse reference is never more than one cm. from the median line. This tendency to localize esophageal sensations upon the soft portions of the body suggests that these portions come by virtue of their mobility to be connected in experience with internal parts.

Cold and warm stimuli in the stomach produce the corresponding sensations, which are identical in quality with cutaneous cold and warmth. It is probable, however, that these sensations do not arise from the stomach itself but from the superficial tissues. Extreme warmth gives rise to pain which may originate in the stomach.

Distension of the stomach gives rise to sensations of dull pressure and, when extreme, of pain.

Electrical stimulation of the stomach gives rise to sensations characteristic of electric shock; the minimal intensities are so great, however, that there is no assurance that the current does not spread to surrounding tissues.

The stomach is sensitive to strong concentrations of alcohol, to oil of peppermint, and to pepper and to mustard in suspension. Hydrochloric acid may produce hunger pains, and in strong concentrations dizziness in the head.

Localization of strong electric shocks within the stomach is quite accurate, much more so than the localization of shocks within the esophagus. The points of reference follow the probable position of the stomach in both the longitudinal and transverse dimensions.

The anus is sensitive to both warmth and cold. The rectum is probably insensitive to both as such; water of 60° C., however, produces pain.

Distension of the rectum produces the call to defecation in all degrees of intensity. The disturbance in this complex experience is so great as to mask any specifically local sensations of pressure that may be set up. In intense degrees the call involves pressures and pains in the abdomen and the genital organs.

The rectum is sensitive to electrical stimulation. It is most sensitive at the anal end.

Alcohol and hydrochloric acid are sensed in the rectum. Oil of peppermint, pepper, and mustard are not. The anus is sensitive to all but the mustard. Hydrochloric acid also produces the call to defecation.

Hot and cold enemata fail to produce thermal sensations in the colon. They do produce pain. Pressures and pains result from distension, but can not be attributed to the intestine itself.

A NOTE ON THE RETENTION OF ACQUIRED CAPACITIES[1]

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It is a familiar fact that aptitudes once acquired, if not indeed inherited tendencies, are weakened by lack of use. *Ex desuetudine amittuntur privilegia*, remarked Friar John to Panurge. The process may be observed at all psychological levels. Some of us may still call to mind the feeling of inadequacy with which, after a student's protracted summer of intellectual inactivity, we again faced the tasks of the laboratory and the lecture-room. It requires a few days to get one's knowledge under discipline again. In the more highly intellectual games, where the objective test of capacity is quite immediate, the necessity for constant practise is fully recognized. When, after a period of neglect, an ordinary player again resumes such pastimes as chess or whist, he is not surprised if he occasionally misses a simple combination or overlooks a not too obvious finesse; errors that are understood, and which at a former period could not have occurred. Such events as these demonstrate how relative a thing is the term "to forget." We may speak of a response as "forgotten" when it is not called up in the presence of some associative milieu that would have called it up at some previous time. But another associative milieu may still call it up, and only when this is no longer possible, is a response absolutely forgotten. Theoretically one might suppose that forgetting begins when immediate learning ceases, but there are experimental observations to indicate that this is not quite the case, there being a short period, 15 seconds or so after the stimuli, during which the effectiveness of the memory increases. The fewer and simpler associations required, the less will naturally be lost. The writer probably plays casino at least as well as when last remembering to have played it, fifteen years ago; auction bridge, not as well as two months ago.

[1] Read at a meeting of the Section of Anthropology and Psychology of the New York Academy of Sciences, February, 1913.

In the motor sphere, losses through disuse may be the product either of less adequate co-ordination, or of deterioration in the muscle. Both seem to have a part. When on the resumption of lawn tennis in the spring, one plays well for a set or so and then goes to pieces through fatigue sensations, the former factor appears to preponderate; and when it takes some moments to again get into the swing of some move with the punching bag, the latter. On the other hand, muscular fatigue is often a secondary result of inadequate co-ordinations; witness how a slight but practised woods-walker can always outlast a sturdier but less experienced companion. Comparison between the retention of intellectual and motor memories is possible with quantitative measurement; it seems also that after voluntary learning has ceased, there is more automatic Einprägung (of the "learning to skate in summer" type) with motor associations than with intellectual ones.

Proper mental or motor response to a situation, once learned, may through disuse, appear as practically entire loss, as partial loss, or as mere delay. In mental processes it is not easy to distinguish between the latter two, because the delay brings about changes in the general associative set in which the particular association required is more likely to arise. Partial loss is more especially a high level process, delay one of low levels. An especially pure form of this delay is seen in the decreased speed of practised simple reaction. A thousand observations with the writer during December, 1906, gave an approximate average of 105 with an m. v. of 10; 48 observations recently made at Harvard, though under different conditions of stimulation, an average of 123 and m. v. of 9.2.[1]

For experimental purposes it seems more desirable to measure these decreases of functional efficiency in terms of delayed responses than in terms of lost responses. That is, to consider responses which have no likelihood of being lost under the experimental conditions, and measure the amount by which the responses have been delayed, from any cause incident to the disuse of a former practise ability. Two series of practise experiments, already described, afforded the conditions for the present observations. The first of these concerned the tapping test, the second the functions of addition and cancellation of digits.

Two subjects had taken part in the practise experiment

[1] The first of these observations was made with Professor Henmon; for the second the writer is indebted to Dr. Langfeld.

with the tapping test, but unfortunately only one remained accessible. The following table presents figures by which the gross rates for thirty seconds in the recent series may be compared with those of 5½ years previous.[2]

TABLE I

NUMBER OF LAPS IN 30 SECONDS FOR 5 SUCCESSIVE SERIES DAILY AT 2′ 30″ INTERVALS

Series	Right Hand						Left Hand					
	Days 1–10, 1907		Days 21–30, 1907		Days 1–10, 1912		Days 1–10, 1907		Days 21–30, 1907		Days 1–10, 1912	
	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.
1″	194.6	3.6	209.5	4.1	205.7	5.5	184.6	3.7	197.3	5.1	186.3	5.5
2″	195.4	4.4	213.7	3.7	208.3	5.8	183.8	4.7	202.3	3.8	189.6	4.0
3″	197.7	3.9	221.5	3.7	216.5	5.5	188.4	6.2	207.7	3.1	195.1	6.3
4″	200.8	3.1	223.6	2.2	218.8	5.8	188.9	4.5	211.9	5.5	198.3	5.6
5″	201.1	5.5	224.9	3.7	221.7	4.7	189.1	4.7	212.4	3.1	201.5	6.9
Av.	197.9	4.1	218.6	3.5	214.2	4.9	187.0	4.8	206.4	4.1	195.2	5.2

Comparing averages for ten consecutive days, there has been but a slight decrease in the efficiency acquired in the earlier experiments. The ten recent ones average not far from the last ten days' practise of 1907. A striking subsidiary fact is that the warming up phenomenon between the successive 30″ series, practically absent until brought out by practise, has been in amount practically unaffected by the five years' intermission. Here the 1912 experiments are practically identical with those of 1907, even to the series showing the greatest warming up gain, the third in each hand. But while during the 1907 practise the left hand gained practically the same as the right, it has during the five intervening years lost very much more, both absolutely and relatively. Probably in a right handed person, the ordinary activities, in which it has the greater share, preserve its specially acquired capacity better than that of the less preferred hand. Finally, the variation of corresponding series on different days is distinctly greater than at any time during 1907. The effect of the 1907 practise was to reduce this variation, so that this is a practise effect that has been more than lost.

To consider the results from the standpoint of fatigue, the subjoined table gives the figures for the six successive 5″ intervals of each 30″ series, tabulated for the same set of data as before:

[2] The earlier results have been reported in this JOURNAL, XIX, 1908, 437-83.

TABLE II

NUMBER OF LAPS IN EACH OF THE SUCCESSIVE 5″ INTERVALS. AVERAGE OF 50 CASES OF EACH INTERVAL, 5 FOR EACH OF TEN DAYS. THE M. V. GIVEN THE DAY TO DAY VARIATION OF THE AVERAGE OF THE 5 CASES FOR THAT DAY.

Series	Right Hand						Left Hand					
	Days 1-10, 1907		Days 21-30, 1907		Days 1-10, 1912		Days 1-10, 1907		Days 21-30, 1907		Days 1-10, 1912	
	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.
0″ - 5″	36.4	1.04	40.8	1.06	39.8	.80	34.2	1.06	38.1	.76	36.3	1.10
5″ -10″	34.7	.52	38.8	.50	37.7	1.00	32.7	.79	36.1	.69	34.3	1.14
10″ -15″	33.3	.60	36.5	.62	35.9	.94	31.2	.72	34.6	.54	32.4	.96
15″ -20″	32.1	.59	35.1	.48	34.4	.70	30.4	.64	33.3	.50	31.2	.82
20″ -25″	31.1	.54	33.9	.51	33.5	.96	29.5	.73	32.4	.56	30.2	.72
25″ -30″	30.6	.32	33.4	.52	32.8	.84	28.9	.46	32.0	.58	29.7	.64

The situation is different in the two hands. The right hand, which as we just saw, has lost less, loses more in the earlier intervals than it does in the later; the left hand has its greater loss in the later ones. In consequence, while the right hand is in the 1912 experiments slightly less fatigable than at the close of the 1907, the left hand is rather more so; that is, it has lost more than the right both in respect to initial maximum and to endurance. The *f*'s express this endurance for the different groups of experiments as follows:

TABLE III

AVERAGE INDEX OF FATIGUE (*b*) FOR THE SUCCESSIVE 10 DAY PERIODS

	Days 1–10, 1907	Days 11–20, 1907	Days 21–30, 1907	Days 1–10, 1912
Rt. Hd.	89.0	88.2	87.0	87.8
Lft. Hd.	89.5	89.2	88.4	87.1

As is seen, the 1907 practise tended to increase the initial rate of this subject at the expense of his endurance; the endurance in the left hand has continued to weaken, while in the right it has been somewhat restored.

Among the most positive results of the 1907 experiments was that the later series of any given day became, as a result of practise, more immune to fatigue than the earlier. This feature is scarcely present in those of 1912, as is seen in the following table:

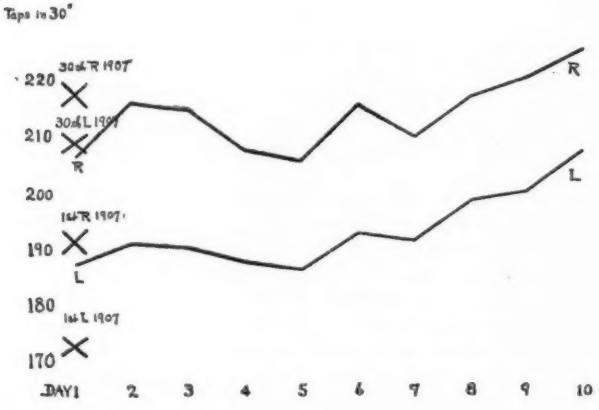
TABLE IV

AVERAGE *f*'S SHOWING COMPARATIVE FATIGUE EFFECTS IN THE DIFFERENT 30″ SERIES

Series	RIGHT HAND						LEFT HAND					
	Days 1-10, 1907		Days 21-30, 1907		Days 1-10, 1912		Days 1-10, 1907		Days 21-30, 1907		Days 1-10, 1912	
1	88.4	2.2	86.1	1.9	87.8	2.0	89.3	1.9	86.5	1.9	85.9	1.5
2	91.0	1.6	86.5	1.7	87.4	1.5	91.2	1.8	89.0	1.4	88.9	1.3
3	88.4	3.5	86.6	1.6	88.2	1.4	89.5	2.7	88.9	1.7	87.3	2.3
4	88.1	1.5	87.5	2.7	88.1	1.0	89.5	2.5	89.3	2.4	87.2	2.0
5	89.3	.9	87.9	1.7	87.5	1.5	88.0	3.2	88.5	1.7	85.5	2.7

What this means is that while the warming up gain of the later series of a day's experiment was in 1907 unequally distributed, being more partial to the final intervals, it is in 1912 much more equally distributed to all the 5″ intervals. In 1907 the initial maximum was about the same for all five series of a day, while the endurance became better; in 1912 the initial maximum also increases from series to series, and the other intervals only in proportion to it. This, then, is another acquired property of the test that disuse has altered.

One thinks of practise as most rapid at the beginning; and one would think that on relearning an acquired but partially disused ability the gain would be especially prompt and rapid. How far this is from being the case here, is shown in the following curve:



There is no evidence of any coming gain until the 6th day; here there was a 10 days' intermission, that showed nothing abnormal. The gain now continues to the very end, and it should be mentioned that after the ninth day an acute illness caused an intermission that was prolonged to six weeks, before the final experiment.

Sensations of fatigue seem to have been more prominent in the 1912 than in the 1907 experiments. They were at first disagreeably so in the left, less marked in the right, and, as natural, decreased with the renewed practise. Their correspondence on different days with the actual fatigue conditions as indicated by f is fairly close, and seems to manifest responsiveness by reflex inhibitions to the sensations of fatigue.

The addition and cancellation tests lack the detailed precision of the tapping test, and in the present problem are subject to an additional disadvantage. The practise effect in the tapping test is relatively small, and above all quite slow. Repeated experiments on different days are therefore possible for the correction of chance variations in a single day. In the other two tests, the practise is rapid and great, so that to repeat them in their previous form involves a day to day practise that destroys the relevancy to the problem, of observations extending over different days. The efficiency of only the first day of practise renewal is to be regarded as representative of the loss by disuse. It would probably have been wiser to divide the experimental work so as to extend the tasks of one day over a period of five; this was not done, however, the experiments being repeated for two days in the same form as they had on the last day of previous practise.

The data are more significant than those of the tapping test, as six of the original ten subjects remained available for them; and an interesting point to note incidentally is that while there is everywhere a persistence of the 1910 practise, the nature of the experiment had been sometimes forgotten. A synopsis of the results is as follows:

TABLE V

	Number of additions in 5 minutes on the 1st experimental day, January and March, 1910.	Number of additions in 5 minutes on 2nd experimental day.	Number of additions in 5 minutes on 30th experimental day, February and April, 1910.	Number of additions in 5 minutes on 31st experimental day, December, 1912.	Number of additions in 5 minutes on 32nd experimental day, December, 1912.	Absolute gain in number of additions from 1st to 30th days.	Absolute loss from February-April, 1910, to December, 1912.	Relative gain in number of additions from 1st to 30th days (per cent. which 30th day is of 1st day.)	Relative loss from February-April, 1910, to December, 1912.
Experimental Day	(1)	(2)	(30)	(31)	(32)	(30—1)	(30—31)	(30+1)	(30+31)
Average of 6 subjects.	234	274	447	343	375	213	105	191	130
Their M. V.....	27	28	75	43	53	54	41	22	10
Per Cent. which M.V. is of Average.....	11.5	10.2	16.8	12.5	14.2	25.4	39.1	11.5	7.7
NUMBER-CHECKING TEST. FOR THE WORDS "NUMBER OF ADDITIONS IN 5 MINUTES," ABOVE, *read* "AVERAGE NUMBER OF 0's CHECKED IN 1 MINUTE, 5 CONSECUTIVE TRIALS."									
Average of 6 subjects.	56[1]	63[1]	107	74	80	51	33	192	145
Their M. V.....	9	7	13	11	10	8	6	13	10
Per Cent. which M.V. is of Average.....	16.1	11	12.2	14.9	12.5	15.7	18.2	6.7	6.9

[1] These figures are the averages of five records made one each on the first and second five experimental days.

In the comparison of losses by disuse we meet here with the same dilemma of the absolute and relative changes. The absolute change appears the better criterion on pragmatic grounds, but both represent real aspects of the change, and both are presented. In the 1910 experiments alone one's interpretation could be somewhat guided by the consideration that the higher a subject is in the scale of practise, the more difficult it is for him to increase his performance by the same absolute amount. In the question of whether superior initial efficiency represented actual practise or practiseability, the results of the 1910 experiments [3] therefore pointed to the latter conclusion. We know that the maximal ability of the 1910 experiments is the result of special practise, but at the outset of the 1912 experiments we have still to discover

[3] Reported in this JOURNAL, XXIII, 1912, 75-88.

whether a person who gains much by practise loses his gain more or less readily than a person who gains but little. We should have perhaps a right to suppose that plasticity will manifest itself in negative as much as in positive directions.

In the addition test, the average drop from the end of the 1910 to the 1912 experiments is 105 units, or not quite half the absolute gain from the beginning to the end of the series of 1910. The mean variation of the 1912 experiments is distinctly less than at the close of 1910. In the absolute amount of practise gain, as well as of later disuse loss, there is much greater relative variation than in any of the daily performances among themselves. The performance at the end of practise is nearly double that at the beginning, and about a third more than that after disuse.

A weak point in the work is that no controls of the quality of vision are available for the different experiments. In one case there is positive report of a better correction of refractive errors in 1912 than obtained in the 1910 experiments. This was in the case of a subject who had shown very slow, though steady, practise effects in 1910; and the somewhat better vision in 1912 would make the loss to 1912 appear less than the fact. However, this subject actually loses on the second day of resumption instead of gaining, and in the number-checking test, the absolute gain here is less than the average.

In the number-checking test, the subjects show almost the same relative practise gain as for the addition test. The tendency to loss by disuse however, is distinctly more, the performance at the end of the 1910 practise being 145% of that at resumption, instead of 130% in the addition test. One subject has lost practically all of the 1910 gain, who had also lost more than any other of the gain in addition.

With the tapping test there is a superficial difference in the situation, for the thirtieth day of 1907 is only 26 taps or 14% ahead of the first one, while the resumption of 1912 shows a loss of 12 taps, the thirtieth day being 106% of this record. However, the disuse loss is thus some 50% of the distance from the end of practise to its beginning; so that while the changes are much less, their proportion is of the same order as the average for the addition and cancellation tests.

These observations introduce two special questions for the data. First, is a subject who gains much by practise also likely to lose much ("easy come, easy go")?

The data do not give a unified answer to this question. In the addition test the situation is fairly clear, large absolute gain going with large absolute loss to a positive correlation

of Pearson 81, that of the relative changes being 58. If the case with relieved eyestrain is excluded, the coefficients become 85 and 49 respectively. In the number-checking test, however, the presence of this case alters to 56 a coefficient that is otherwise -20,while in the relative changes a negative coefficient of -33 results from this case and that previously mentioned who lost all the gain, which was relatively little. If the former case is excluded, the coefficient is -19, if both, it is distinctly plus. It is by no means uninstructive to observe these extreme departures from the supposedly normal tendency. The probable meaning of the results is that while, when the practise curve follows a normal form, a loss by disuse may indeed be expected in proportion to the amount of practise gain, "irrelevancies" bring about anomalous forms of the practise curve in special cases, where such relationship does not appear.

A further question concerns the likelihood of a subject who loses much in one function to also lose much in the other. As measured by the Pearson coefficient, this seems to be strongly the case, more so with the absolute losses than the relative ones. In this connection it is noteworthy that if one compares the ground covered between the beginning and end of practise in the 1910 experiments, one finds a negative correlation in the improvability of the two tests, which in the relative gains is extreme. Other data do not show this negative relationship so consistently, and it is probably a good deal slighter than appears here. Still, the interesting result is reached that disuse loss is a more generalized property of the organism than practise gain. The capacity to acquire may be high for some abilities and low in others, but the tendency to forget them seems to be more equal for different abilities. The figures illustrating this point are,

Correlation between Addition test and number checking test,	r
absolute practise gain	−30
absolute disuse loss	78
relative practise gain	−95
relative disuse loss	64

In sum, such observations emphasize the relativity of certain knowledge, and of memory. Pragmatically, to know a thing is to have established an association or co-ordination path which causes us to react according to it. We know a thing more certainly according as we react more certainly or in any way more effectively as though that thing were so. Though the certainty has a different source, you do not know that

two and two make four more certainly than the dervish knows the prophet's paradise awaits him on death in battle for his faith. To say that one takes a few sigma longer to add six and five than one did two years ago, is another way of saying that he does not know their sum so well; and this same lessened decisiveness of response, many times magnified, finally produces false reactions as in the method of right and wrong cases. We lose the knowledge that six and five make eleven just as we lose the knowledge of how to effectively return a service at tennis, or of how to make 230 taps in 30 seconds. Our tendency to lose in these accomplishments also appears more generalized than do our abilities to acquire them. Another feature of this sort of deterioration is its relative independence of positive interference from without, for no considerations enter to make what were the proper responses before, not the proper ones now. In this respect the work falls short, as most of experimental psychology does, of meeting the conditions of actual life, where success is a matter not simply of knowing, but of knowing the right things.

ON PSYCHOLOGY AS SCIENCE OF SELVES

By Josephine Nash Curtis[1]

The Advancing Claims of Self-psychology

Self-psychology was at first brought forward by Professor Calkins as one of the possible points of view in psychology. In "Psychology as Science of Selves" (1900) she writes: "This essay [insists] . . . on the equal validity of two underlying forms of psychology . . .. The first of them may perhaps be named 'Atomistic Psychology'. It treats of . . . psychic phenomena, considered without reference to any self, and its primary procedure is the analysis of these psychic facts into irreducible elements and the classification of complex phenomena according to the preponderance of given elements. For the second of these ultimate forms of psychology I have vainly sought a satisfactory name. It is distinct from the science of the bare psychic contents, and has been variously regarded as a study of conscious functions, of mental operations, and of activities of the self. Most simply and with adequate recognition of the profoundly social nature of the self, this form of psychology may be treated as the science of conscious selves."[2]

In the "Introduction to Psychology" (1904) we find similar statements. "The book is written in the conviction that psychology should study consciousness, both as a series of complex mental processes, or ideas, and as a relation of conscious selves to each other "[3]; and, again, "We defined science as the study of facts or phenomena, that is of limited bits of reality, taken for granted without investigation of their relation to the whole of reality. Now it is certain that consciousness, or ideas, regarded without reference to a conscious self, may form the material of a scientific psychology; and some psychologists have limited the science to the study of these momentary contents of consciousness, not regarded as the experiences of a self. But it is equally evident, in the opinion of the writer,

[1] From the Psychological Seminary of Cornell University.

[2] M. W. Calkins, Psychology as Science of Selves, *Phil. Rev.*, 9, 1900, 490.

[3] *An Introduction to Psychology*, 1904, v.

that selves also may be treated as facts or phenomena, because they are taken for granted by everyday people, without inquiry about their relation to 'reality.'"[4]

In "Der doppelte Standpunkt in der Psychologie" (1905) Miss Calkins again urges her position. "Psychologically we may consider consciousness from two points of view: we may conceive consciousness either as a series of linked psychical processes, wholly without reference to the conscious I, or as a many-sided consciousness of the real I in its relations. In accordance with these two points of view we have a twofold psychology: process-psychology and I-psychology. Every consciousness can be looked at from both points of view: in other words, both methods can be consistently worked out."[5]

So far (1905) the point of view of self-psychology has been urged on the ground that it is as valid a form of psychology as 'atomistic' (or 'structural') psychology. Now, however, we find it offered as a mediator, a reconciliation between other forms of psychology. In "A Reconciliation Between Structural and Functional Psychology" (1906) Miss Calkins writes: "I shall . . . try to show [that] that psychologist's self is rightly conceived as fundamental both to structural and to functional psychology, and that it should therefore be studied by both methods."[6] The conception of the self, we read, "facilitates, and necessitates a union between structural and functional psychology."[7] The argument is that: "(1) structural psychologists and one group (but one group only) of functional psychologists are unjustified in their doctrines of the basal psychic phenomenon; but that (2) in their doctrines of psychological analysis, both structural and functional psychology are right: the analysis of the one supplements that of the other . . . (3) . . . both sorts of analysis, structural and functional, are essential to an adequate self-psychology." In greater detail, Miss Calkins argues as follows: "Structural psychology consists essentially in the teaching that the task of psychology is first, to analyse typical experiences until one reach irreducible elements, and second, to classify the ordinary sorts of complex experience according as one or another of these elements predominate . . . Now it is past doubt that this structural analysis of a psychic state is always possible . . . But this analysis into structural elements—it must be insisted—is not necessarily the analysis of an idea or psychic

[4] *Op. cit.*, 156.
[5] *Der doppelte Standpunkt in der Psychologie*, 1905, 32 f.
[6] *Psych. Rev.*, 13, 1906, 61 ff.
[7] *Op. cit.*, 68.

event: it is, on the other hand, quite as easy to discover the structural elements regarded as experience of a self, for though every conscious experience is some relation simple or complex, of a self to its environment, it is also a complex of different elemental experiences, sensational, affective and the like . . .. In a word, psychology as science of conscious and related selves may and should undertake the analytic discovery of elements of conscious experiences as such. By virtue of this structural procedure it is truly a structural psychology, though it utterly repudiates the doctrine of the psychic event, or idea as the basal fact of psychology." After some discussion Miss Calkins concludes that "functional psychology, rightly conceived, is a form of self-psychology, that its basal phenomenon is the psychologist's self, and that its significant contributions to psychology are, first, its doctrine of the inherent relatedness of self to environment, and second, its insistence on the progressive efficiency or utility of these relations . . .. Pruned of . . . biological excrescences, a functional psychology . . . is a self-psychology."[8]

Self-psychology, then, has been offered first as a possible form of psychology; and next as the form which "harmonizes the essential doctrines of a structural and of a functional psychology."[9] As we might expect, out of the latter conception comes the belief that self-psychology is a form of psychology superior to the others. This idea is developed to some extent in the article just cited. Miss Calkins says: "I shall aim to show . . . first that these actual (concrete conscious) experiences cannot adequately be described by enumerating their structural elements, and second, that the conception of them as relations of self to environment involves or allows all the teachings essential to functional psychology." In a later article[10] is a more insistent statement: "I am myself profoundly convinced that consciousness is never adequately conceived except as conscious self; and that both the other conceptions of consciousness [structural and functional] imply this." The inadequacy of structural psychology is stated as follows: "If I conceive psychology as science of ideas I inevitably raise the scientifically relevant question:[11] Whose idea? and then I

[8] *Op. cit.*, 75.
[9] *Op. cit.*, 76.
[10] *Journ. of Phil., etc.*, 4, 1907, 677 f.
[11] It may be suggested here that Miss Calkins must be generalizing from her own experience. Many psychologists have conceived psychology as "science of ideas" without raising the question; indeed, these psychologists see no more necessity for raising it than the physiologist sees for asking "Whose muscle?" or the geologist "Whose stone?"

arbitrarily refuse to answer my own question. In other words, the 'idea' is immediately experienced as idea of a self, or subject, mind, ego—call it as one will. To refuse to deal with this self is indeed theoretically possible, but is a needlessly abstract, an artificial, an incomplete procedure."[12] Moreover, "an idea psychology is incapable, through this fundamental inadequacy, of describing some, at least, of the facts which it studies." Functional psychology, also, is inadequate. We find that "narrowly scrutinized, the theory of psychology as science of mental activities turn out to be a needlessly abstract, an arbitrarily inadequate view. For activity is clearly a character of something . . . One inevitably and rightly asks concerning any character not only 'of what sort is it?' but 'whose is it?' . . . In truth, the conception of mental activity requires the conception of mental actor, even more obviously than the full conception of the idea includes that of its possessor."[13]

The superiority of self-psychology is still further set forth in Miss Calkins' "First Book in Psychology" (1910). In the preface we read: "This book has been written in the ever strengthening conviction that psychology is most naturally, consistently, and effectively treated as a study of conscious selves in relation to other selves and to external objects—in a word, to their environment, personal and impersonal . . . I have . . . abandoned . . . [the] double treatment [of psychology both as science of selves and as science of ideas] . . . not because I doubt the validity of psychology as study of ideas, but because I question the significance and the adequacy, and deprecate the abstractness of the science thus conceived."

In the last edition (1914) of this book we find many indications not only that self-psychology is more consistent than other forms of psychology, but also that the change in point of view has rendered former expressions inadequate. One example of this occurs in the preface.[14] "If I were writing this book *de novo* I should throughout refer to the sensational, affective, and reflective elements in such a way as to emphasize the fact that they are found through analysis of consciousness conceived as relation of self to object. Thus I should use the expressions 'seeing colors,' 'hearing tone-qualities,' 'liking,' 'distinguishing,' along with, or even in place of the parallel expressions, 'visual and auditory qualities' . . ."

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[12] *Op. cit.*, 678 f.
[13] *Journ. of Phil., etc.*, 682.
[14] xii f.

We have now outlined in brief the advancing claims of self-psychology, first as a possible, then as a reconciling, and last as the most natural, consistent and effective form of psychology. With this introduction to the subject, we shall consider the system of self-psychology; and shall discuss in order its subject-matter, method, problem, and results; for it is with reference to these points that any scientific system must be appraised. The subject-matter of self-psychology is, of course, the self; and it is to the self-psychologist's self that we shall first turn our attention.

The System of Self-Psychology

The Concept of the Self

Miss Calkins' conception of the self is not clear. In the most general way, her self is a fact, a phenomenon, that stands at the same level as qualities, things, moments, events. The self is also conceived as having characters. Although Miss Calkins follows her general statements immediately with her discussion of the characters, for the sake of clearness we shall first treat of the general concept and then, in another section, of the characters.

The self is a fact, in the same category with things and events. It may be viewed in the light of various metaphysical systems, but "no one of these . . . systems precludes the scientific treatment of selves."[15] These statements, of course, tell us nothing definite. A further search reveals that "besides realizing my conscious experiences, or feelings, I am also conscious of my conscious self, as in a sense including, but not as identical with, the perceptions, the emotions, or the thoughts of any given moment;"[16] and in "Der doppelte Standpunkt" we read: "Self-consciousness can—at least partially—be described from two sides. We can first analyse the self-consciousness as well as the psychical processes into so-called elements."[17] Miss Calkins recognizes three sorts of "structurally elemental consciousness:"[18] sensational, affective, and relational. But in what sense, we may ask, does my conscious self include the perceptions of the moment? It is evident that, for Miss Calkins, the self is not merely the sum of its perceptions, emotions, and the like; but we are not told what it is in addition to these, or what it is instead of the sum-total of these. She adds, however, that "with the exception of the

[15] *Phil. Rev.*, 492.
[16] *Introduction to Psychology*, 151 f.
[17] 34 f.
[18] *First Book*, 1914.

analysis into structural elements, the only description of self-consciousness is, first, as consciousness of myself contrasted with other selves, and second, as consciousness of my varying relations or attitude to these other selves."[19] Unfortunately, this statement does not aid us; for instead of continuing the line of thought and telling us the differences between ourself and other selves, she considers only the consciousness of "my varying relations to other selves." As we are not, at present, interested in the relations of the self, but merely in the self itself, we must continue the search for information concerning it.

In "Der doppelte Standpunkt" we read: "This I, the self or subject, cannot, of course, be defined, for it is the most intimate, most fundamental thing that we know, and, on that account cannot be reduced to other terms. The I is simply the I; everyone knows for himself what it is . . . We can first analyse the self-consciousness . . . into so-called elements . . . In the second place, we know that the real self or I is known in contrast and in relation to other selves; so we reach the important conclusion that each I is essentially a social I."[20] There are various comments to be made on this passage. In the first place we must acknowledge that, if it is true that the self cannot be defined, we are unreasonable in expecting Miss Calkins to define it. But as she distinguishes between selves and "things," she should be able to point out the differences (attributive or other) between the two. In the second place, it is surely not true that everyone knows for himself what the I is. The actions of the plain man may be *interpreted* to mean that he knows what the I is,[21] just as his choice of language in speaking of the sun might be interpreted to mean that he knows the sun revolves about the earth. Yet astronomy would be no science if it took as its fundamental fact that the sun revolves about the earth, just because everybody says that it does. What answer can Miss Calkins make to the person who says: "I do not know what the I is?" In the third place, it is no assistance to one hunting for a description of the self to say: "the real self is *known*

[19] *Introduction to Psychology,* 151 f.

[20] 34 f.

[21] The writer took occasion recently to ask a number of persons, none of them students of psychology, exactly what they meant by the I. The answers were marked by variety and evasiveness; and the majority might be simmered down to the one given by an intelligent workingman after a day's meditation. This was the answer: "Conundrum, ain't it?" The offhand appeal to the 'plain man' of the philosophers is, indeed, a curious recourse for the scientific psychologist. Why not collect observations?

in contrast and in relation to other selves." Aside from the circularity of the statement, it is no *description* to say the self is *known as related. What* it is that is known, how it is known as related, and under what conditions it is known as related, are not stated. And, for one, the writer must assert that either she does not understand the expression or else she does not know herself as related, except that as the result of a process of logical reasoning she may conclude that she must be related.

In "A Reconciliation Between Structural and Functional Psychology," Miss Calkins writes: "By self as psychic fact I mean what the plain man means by self, in so far as this does not involve the view that body constitutes part of a self. This conscious self, the plain man's self, in the developed form in which we commonly study it, is in the first place realized as underlying the experiences of the moment—as having percepts, images, and the like, or more exactly, as 'perceiving,' 'imagining,' and 'feeling.' And, in the second place, every self, besides being fundamental to its own ideas or experiences, is also a related self. That is to say, I am always conscious of myself as in some way related to my environment social or physical."[22] It is undoubtedly true that the "plain man's self," if it be realized at all, may be "realized as underlying the experiences of the moment" and as being related to environment. But in that case is not the "plain man's self" decidedly metaphysical in character? Is not what he considers the "self of selves" the part which he expects to live, his immortal soul? There must be some foundation for the expression, so common in everyday life, "trying to keep soul and body together." Taken at its face value, the expression implies that a self has two parts: body and soul. Indeed, the other common expression "mind, body, and soul" and the corresponding "physically, mentally, and spiritually" imply a three-part self. Miss Calkins has intentionally omitted the body from her "plain man's self;" but she has also omitted the soul, or spirit, the part of the self which the "plain man" apparently considers the most important.

Let us sum up what we have learned so far about the nature of the self. (1) The concept of the self, as given to us by Miss Calkins, is far from clear; (2) beside structural analysis we must describe the self in terms of the difference between it and other selves—without having these differences stated; (3) the self cannot be defined; (4) by self Miss Calkins means the plain man's self, but she has deprived him of his body and soul.

[22] *Psych. Rev.*, 13, 1906, 63 f.

All these general statements concerning the self really tell us nothing. But before we go on to consider the characters which Miss Calkins ascribes to the self, we shall do well to examine what seems to be the proximate source of this self, and see if that clears the concept at all.

James' Concept of the Self

In his "Briefer Course" James gives the fundamental thought of self-psychology. "Whatever I may be thinking of, I am always at the same time more or less aware of *myself*, of my *personal existence*. At the same time it is *I* who am aware."[23] In the "Principles of Psychology" we read such passages as: "The universal conscious fact is not 'feelings and thought exist' but 'I think' and 'I feel';"[24] "Our own bodily position, attitude, condition, is one of the things of which *some* awareness, however inattentive, invariably accompanies the knowledge of whatever else we know. We think and as we think we feel our bodies as the seat of the thinking . . . *Whatever* the content of the ego may be, it is habitually felt *with* everything else by us humans;"[25] "All people unhesitatingly believe that they feel themselves thinking . . . I regard this as the most fundamental of all the postulates of psychology."[26] When we ask what this self of James' really is, we find that, in the widest possible sense, it is the sum total of all a man can call his; "not only his body and his psychic powers, but his clothes and his house, his wife and children, his ancestors and friends, his reputation and works."[27] We find also that there are various constituents of the self, which may be divided into two classes: the Empirical self, made up of the Material Self, the Social Self, and the Spiritual Self, and the Pure Ego. The characters of these selves are given in some detail: "The body is the innermost part of the Material Self in each of us . . . The clothes . . . Our immediate family . . . Our home . . . A man's Social Self is the recognition he gets from his mates . . . Properly speaking a man has as many social selves as there are individuals who recognize him and carry an image of him in their mind . . . A man's fame, good or bad, and his honor or dishonor, are names for one of his social selves . . . By the Spiritual Self, so far as it belongs to the Empirical Me, I mean a man's inner or sub-

[23] W. James, *Psychology*, 1905, 176.
[24] W. James, *The Principles of Psychology*, 1890, I, 226.
[25] *Op. cit.*, 241.
[26] *Op. cit.*, 185.
[27] *Op. cit.*, 291.

jective being, his psychic faculties or dispositions, taken concretely; not the bare principle of personal Unity, or 'pure' Ego . . . These . . . are the most enduring and intimate part of the self."[28] James then goes on to discuss this "self of all the other selves," and concludes that everybody would call it the "active element in all consciousness." "This central part of the self is . . . no *mere* summation of memories or *mere* sound of a word in our ears. It is something with which we also have direct sensible acquaintance." Upon careful examination, this "self of selves" proves for James to consist mainly of "peculiar motions in the head or between the head and throat."

We find, then, in James the fundamental thought of self-psychology: that "I am always more or less aware of myself." A corresponding statement is made by Miss Calkins: "I am always, inattentively or attentively, conscious of the private, personal object, myself, whatever the other objects of my consciousness."[29] On the face of them, these two statements seem almost identical; but in reality they are quite different. The self of which James was "always more or less aware" was mainly, motions in the head; that of which Miss Calkins is "always . . . conscious" cannot be defined. James' self is carefully differentiated from the other selves, the selves which seem important to the plain man; Miss Calkins' self *is* the plain man's self (*minus* body). We find in James a detailed discussion of what we feel when we become aware of the existence of the 'self of selves;' we find no such detailed discussion in Miss Calkins, but are told only that the self is "immediately experienced as possessed of characters."

It is evident that the fact that James gives us the fundamental thought of self-psychology does not make James a self-psychologist. In the first place, we find in James several exceptions to the ever-present awareness of self. He says: "A mind which has become conscious of its own cognitive function, plays . . . 'the psychologist' upon itself. It not only knows the things that appear before it; it knows that it knows them. This stage of reflective condition is, more or less explicitly, our habitual adult state of mind. It cannot, however, be regarded as primitive. The consciousness of objects must come first. We seem to lapse into this primordial condition when consciousness is reduced to a minimum by the inhalation of anaesthetics or during a faint."[30] He then quotes

[28] *Op. cit.*, 292 ff.
[29] *First Book*, 1914, 5.
[30] *Principles*, 272.

Herzen to the effect that "at the beginning of coming-to, one has at a certain moment a vague, limitless, infinite feeling—a sense of *existence in general* without the least trace of distinction between the me and the not-me," and gives various other instances in which no consciousness of self is present. The statement that I am always "more or less aware of myself" must be changed for James to read "The normal human adult under normal conditions is always more or less aware of himself." Self-psychology, on the other hand, is founded on the postulate that the statement is *always* true for man, and is true for babies and animals as far as they are conscious at all.[31] Again James seems to disagree with the first principles of self-psychology when he says: "Many philosophers . . . hold that the reflective consciousness of the self is essential to the cognitive function of thought. They hold that a thought, in order to know a thing at all, must expressly distinguish between the thing and its own self. This is a perfectly wanton assumption, and not the faintest shadow of reason exists for supposing it to be true. As well might I contend that I cannot dream without dreaming that I dream, swear without swearing that I swear, deny without denying that I deny, as maintain that I cannot know without knowing that I know. I may have either acquaintance-with, or knowledge-about an object *O* without thinking about myself at all. It suffices for this that I think *O* and that *O* exists. If, in addition to thinking *O*, I also think that I exist and that I know *O*, well and good."[32] But Miss Calkins says that "to be conscious is to be conscious of my conscious self." A third difference between James and the self-psychologist is that James "notices" the characters of thought, whereas Miss Calkins "immediately experiences" the characters of the self. We need not stop here to discuss this point, as the question of the immediate experiencing of the characters of the self will be considered in a later section of the paper. The fourth difference between James' and Miss Calkins' selves has already been suggested: the fact that James' self is described in terms of sensation, while Miss Calkins' self cannot be expressed in terms either of idea or of function, but is immediately experienced as having certain characters.

Our consultation of James has brought out the fact that, although he gives the fundamental thought of self-psychology, the self of which he speaks is not the self of which Miss Calkins speaks. If we try to equate Miss Calkins' self with one of James', we find, first, that her self cannot be the Material

[31] *Journ. of Phil.*, etc., 5, 1908, 68.
[32] *Principles*, I, 274.

Self and cannot be the Social Self; neither can it be the pure Ego. If, then, it is any of James' selves, it must be the Spiritual Self. At first glance, it seems to resemble this self; only that James discovers "motions in the head," while Miss Calkins comes out with nothing of the sort. We must, therefore, conclude that James, although he gives us a perfectly clear idea of his self, helps us very little toward understanding Miss Calkins' self.

The Characters of the Self[33]

It is hardly fair to Miss Calkins, however, to give up the search for the real nature of the self until we have examined her account of its characters; for, as she says, we are not merely always conscious of ourselves, but we are also conscious of ourselves as possessing certain characters. These characters, moreover, are said *not* to belong to idea or to function,[34] and so are employed to distinguish the self from idea and function. In the "First Book"[35] we read: "The conscious self . . . is immediately experienced as possessed of at least four fundamental characters. I immediately experience myself as (1) relatively persistent . . . as (2) complex . . . as (3) a unique, irreplaceable self . . . I experience myself as (4) related to (or, conscious of) objects either personal or impersonal."[36]

The Persistence of the Self

The first character of which Miss Calkins treats is persistence. The self, we read, is experienced as "in some degree," "in some sense," "relatively" persistent; "in other words, I am in some sense the same as my childhood self."[37] By persistence

[33] The characters of the self are discussed by Miss Calkins in *An Introduction to Psychology; Der doppelte Standpunkt in der Psychologie; Journ. of Phil.*, etc., 5. 1908, 64 ff.; and *First Book in Psychology.*

[34] *Journ. of Phil.*, etc., 65.

[35] 1914,3.

[36] There is a passage in James (I., 225) which seems to suggest, at least, the characters which Miss Calkins ascribes to the self. "How does [thought] go on? We notice immediately five important characters in the process. . . . (1) Every thought tends to be part of a personal consciousness. (2) Within each personal consciousness thought is always changing. (3) Within each personal consciousness thought is sensibly continuous. (4) It always appears to deal with objects independent of itself. (5) It is interested in some parts of these objects to the exclusion of others." Here certainly may be found the germs of persistence, complexity, and relatedness.

[37] *First Book*, 3.

is not meant ultimate self-identity, but the "kind of identity of which one is immediately conscious."[38]

What precisely do the above statements mean? A translation of the sentence "I am conscious of myself as relatively persistent" might read "I, the present self, am continually conscious of a past self." I, in a sense, *i. e.* in so far as I go by the same name, in so far as I have some of the same ideas, habits, relationships to friends, in general in so far as I have the same functional relations to my environment that I formerly had, am still the same I. But is it not just as true that, in a sense, *i. e.*, in so far as I have different functional relations to my environment, behave differently in the face of the same objective situations, have different ideas, different habits, and so on, I am a different I? Do I not, in other words, just as often, just as emphatically, and just as immediately experience myself as different from my childhood self? And if the first consideration means that self is persistent, does not the second mean that self is at the same time, or in another sense, just as non-persistent as it is persistent? 'Relatively persistent' surely means 'not exclusively or absolutely persistent.' The positive logically involves the negative statement; partial sameness necessarily involves partial difference. Non-persistence, therefore, has the same claim to be a character of the self as has persistence.

But not only may we question whether persistence has a better claim than non-persistence to be considered a character of the self; we may also ask if persistence is *exclusively* a character of self. Miss Calkins dismisses Angell's statement that "general functions like memory (are) persistent" with the remark that "if one scrutinize the real meaning of this statement 'memory—or reason—or will is a persistent function' one finds it to be simply this, that one and the same self at any time may remember or reason or will."[39] Why does she not say that it means that one and the same self *at all times does* remember *and* reason *and* will? That surely seems to be the meaning which Angell intends. Mind for the functional psychologist is a general function (for example, accommodation to the novel) within which less inclusive but still quite general functions (memory, thought, etc.) may be discriminated. Never in the concrete do we find any single general function active in isolation; but, wherever we find consciousness, there we find all these general functions; and in so far

[38] *Journ. of Phil.*, etc., *loc. cit.*
[39] *Ibid.*

as consciousness is persistent, in *so* far are the functions persistent.

Persistence, then, is no more a character of the self than is non-persistence, nor is self to be distinguished from function by means of this character. On the contrary, self appears to be merely another name of the all-inclusive mental function.

The Uniqueness of the Self

We come now to the question of the uniqueness of the self, and of the relation of this uniqueness to the persistence of the self. We find that "I immediately experience myself . . . as a unique, irreplaceable self—I am closely like father, brother, or friend, but I am, after all, only myself; there is only one of me."[40] "This uniqueness is . . . experienced most clearly in our emotional and volitional consciousness: when we reflect upon it we may describe it as a consciousness of a this-which-could-not-be-replaced-by-another. Now we simply are not conscious of ideas and functions as, in this sense, unique. A given self, with a different idea, is still this self; whereas a given idea is this or that idea according as it belongs to this or that self. I am I whether I see or hear, whether I fear or hope, but another self's vision or fear, however similar, is not this experience, but another."[41]

But *why*, we may ask, is a self with a different idea still the same self? Miss Calkins, herself, as we shall see (p. 82), insists that one essential, never-to-be-neglected character of the self is the having, among other things, of ideas. Is it, then, that the *having* is the same whatever the idea is that is had? Is it the same thing to have one idea as to have another? May there be a having without an idea had? May we, so far as self is concerned, abstract from idea but not from the *having of ideas?* Is a self without any ideas possible? Are we in dreamless sleep, *e. g.*, still self-conscious? These and other questions, for which we find no answer, are at once suggested. Certainly, the concept of having and the other concepts so freely used need clarification and definition. Certainly, if (as Miss Calkins maintains in another place) "there can be no objection to considering ideas as part of the self,"[42] then a change of a part means a change of the whole, and the self with another idea is not, and cannot be regarded as, the same self.

"A given idea," says Miss Calkins, "is this or that idea

[40] *First Book*, 3.
[41] *Journ. of Phil.*, etc., 66.
[42] *Ibid.*

according as it belongs to this or that self." In the sense of a structural psychology,—and it is in this sense that Miss Calkins on her own assertion speaks of ideas,—the essence of ideas does not consist in their being had by a self, but rather in their attributive determinations. If ideas differ *only* in respect to what self has them, then all the ideas of a given self are identical, or a given self can have only one idea. In fact, ideas, in the sense both of structural psychology and of the passages of Hume which Miss Calkins quotes in her own behalf in another connection, are themselves unique. "Another self's vision or fear, however similar, is not this experience, but another," says Miss Calkins. True, but not the whole tale! In exactly the same way, *my present* vision or fear, however similar to my *past* vision or fear, is not my past vision or fear, but another. That is Hume's story. And what holds of the uniqueness of ideas holds also of functions. Why is not a remembering-now a not-to-be-replaced-by-another remembering? I can never remember at this particular now again; I never did remember at this particular now before. But the case is not yet ended; uniqueness is not peculiar to ideas and functions. A given chair is unique, every hair on my head is unique; they 'cannot be replaced by others.' In the most general way, every concrete object, every actually existing thing in the world is unique.

And the question now arises, can a self be unique and not persistent, or persistent and not unique? The plain man probably considers himself unique. But he does not talk as if he considered himself persistent when he says, for example, of himself or another, that he "lost himself," "forgot himself," "could not recognize himself," "was not himself," "was out of himself."[43] These expressions sound, at any rate, as if the plain man's self were for him an ideal and not a real persistent being. Moreover, there are times when we become so absorbed in a book or play that for the time being we, to all intents and purposes, *are* the hero, say, and apparently have no consciousness of our ordinary self. Granted that in such a case we are unique; are we also persistent? If we are persistent because we are in some sense the same as the childhood self of the hero whom we for the moment are, then we must be any number of persistent selves. But this cannot be; for we are, by hypothesis, unique: "I am closely like father, brother, or friend, but I am, after all, only myself; there is only one of me." But if I can be Hamlet, why can I not at some other time be "father, brother, or friend?" Or, in

[43] E. B. Titchener, *Text-book*, 1910, 13.

general: is this self a this-not-to-be-replaced-by-another, if we are at various times various selves? Miss Calkins has answered the question of "losing oneself" in a book or play by saying: "Selfconsciousness is surely present when a man seems to himself 'a person' even though another person. He is unconscious . . . of circumstances, of surroundings, of the past, but not of self."[44] But he cannot be conscious of self (according to Miss Calkins) without being conscious of that self as persistent, complex, unique, and related. If he is conscious of himself *in this situation* as persistent and unique, then surely he may have as many selves as he desires, or has thrust upon him, each one of them unique and relatively persistent; a curious condition for the plain man's self!

Uniqueness, then, cannot be a distinguishing character of the self, because ideas and functions and all concrete objects are also unique. In the second place, it is difficult to apply the characters of uniqueness and persistence together to all the situations in which the self is found.

The Complexity of the Self

A third character of the self is that it is "inclusive; it is, in other words, a complex of ideas, functions, experiences."[45] "I am a perceiving, remembering, feeling, willing self."[46] "This is the character of the self which gives to the idea and function conceptions of psychology their hold on psychological thought; for against ideas or functions regarded as parts, or aspects, of the self no crucial objection need be urged."[47]

Whatever the concept of the self, there can be no doubt that complexity is one of its characters. But Miss Calkins has neglected, in the case of complexity, to prove the thesis with which she started: namely, that the characters of the self do not belong to idea or function. She makes no attempt to prove that ideas and functions are not complex; and it is evident that no such proof could be offered, for, in this sense, we have no simple experiences. Complexity, therefore, cannot be used as a distinguishing character of the self.

The Relatedness of the Self

We come to the fourth and last character of the self, its relatedness. "I immediately experience myself . . . as related to (or, conscious of) objects either personal or impersonal. For example, I am fond of my mother . . . and I am tasting

[44] *Psych. Bull.*, 9, 1912, 25 ff.
[45] *Journ. of Phil.*, etc., *loc. cit.*
[46] *First Book, loc. cit.*
[47] *Journ. of Phil.*, etc., *loc. cit.*

an orange . . . It would be impossible to enumerate all the 'personal attitudes' or relations of self to its objects. The following, however, are fundamental . . . receptivity and activity; sympathy . . . attention."[48] This character is the keynote of Miss Calkins' whole system of psychology; for we find that the full definition of psychology is "the science of self in relation to environment"[49] or the "science of the self in relation to, or conscious of, its environment."[50]

In the "Introduction to Psychology" we have: "I, who read this paragraph . . . simply cannot be conscious of my own self except as related in the most varying ways to a vast number of other people. Let one try to drop out of the consciousness of oneself the realization, however vague, of some or all of these relations, the consciousness that one is son, brother, member of a fraternity, student at a university, citizen of the United States: such an imagined elimination of the consciousness of his social relationships leaves a man, in truth, with nothing which he can recognize as himself." In the 1914 edition of the "First Book" Miss Calkins states that she uses the term consciousness "as synonym for 'personal attitude' or 'the self's relatedness to its objects.'"[51] Now, if one define consciousness as relatedness to environment, then it must of course be true that one is conscious of all of one's relations. So if we accept Miss Calkins' definition of consciousness,[52] we must accept her statement as to the relatedness of the self. But it is curious to "immediately experience" four characters of the self, and then to find that one of these characters really includes the other three. Yet, under the above definitions, this is evidently the case with the character of relatedness. The self must be "conscious of" the persistence, the complexity, and the uniqueness of the self; how else can these characters be known? But to be conscious of means to be related to, and so we come back to the fourth character. What are the persistence, uniqueness, and complexity of the self other than ways in which the self may be *related?* Let us first consider persistence. Why should not the character of persistence be included under that of relatedness? By 'rela-

[48] *First Book*, 1914, 3.
[49] *First Book*, 1910, 273.
[50] *First Book*, 1914, 1.
[51] xii f.
[52] This definition of consciousness, when considered with reference to the character of relatedness, presents certain difficulties: for example, if we try to substitute words which Miss Calkins uses as synonyms we become confused. The sense of the following is scarcely clear: "I am related to myself as related to objects."

tively persistent' I mean "I am in some sense the same as my childhood self." Does that not also mean that "I experience myself as in some way related to my childhood self?" In the second place, how can I experience myself as unique, as "closely like father, brother, . . . but after all, only myself," without differentiating myself from father and brother? But I surely cannot differentiate myself from others without experiencing myself as in some way related to them. And as for the complex character of the self, I cannot again experience myself as "perceiving, remembering, feeling, willing self" without experiencing myself as perceiving, etc., something, that is, as *related to* an object, personal or impersonal. But we need not continue, since Miss Calkins herself writes:[53] "The relatedness of a self . . . may be shown to be a character of all consciousness."[54]

We find, further, apparent inconsistencies in the treatment of the different characters. For example, in the 2nd edition of the "First Book" the structural elements are assigned to the complex character: "The mental reduction of this complex experience to its lowest terms gives what are called the structural elements of consciousness;"[55] while in the 4th edition they are assigned to the character of relatedness: "It is possible by attentive introspection to detect within every . . . relation of the self to object, certain elemental (that is, further irreducible) factors or constituents . . . the 'structural elements' of consciousness," and: "From this follows the conception of the sensational, affective and relational 'elements' . . . as constituents of all forms of the relatedness of self to objects."[56] In spite of such statements as the last two, Miss Calkins makes complexity a separate character of the self, and further defines it by saying "I am a perceiving, remembering, feeling, willing self." But surely the 'structural elements' come from the perceptions, etc.; so that they must come from both complexity and relatedness. This result reinforces our former conclusion that the character of relatedness really includes the other three characters.

We have now considered the four characters of the self; have shown that persistence is no more a character of the self than non-persistence is; have shown that some functions at least are persistent; that ideas and functions as well as selves

[53] This statement reads as if persistence, uniqueness, and complexity were *not* characters of all consciousness, as they have at other times been stated to be.

[54] *Phil. Rev.*, 17, 1908, 272 ff.

[55] 14.

[56] xii.

are unique, and that therefore the character is not a distinguishing one; that a self cannot always be both unique and not non-persistent; that ideas and functions as well as the self are complex; that, given Miss Calkins' definition of consciousness, the self must be acknowledged as related; but that, under the definitions of the various characters, persistence, uniqueness, and complexity must be accepted as forms or modes of the fourth character, the relatedness.

The conclusion is that Miss Calkins' conception of consciousness and her conceptions of the characters of the self move in a circle. We start with consciousness as the relatedness of the self, immediately experience the self as possessed of four characters which, as we have seen, all reduce to one, namely, to relatedness and so are back at the starting point without appreciable gain.

The Self as Knower and as Known

There is a further point which Miss Calkins does not make clear; namely, the relation between the self as knower and the self as known. We have every right to ask for this distinction, because throughout the system it is implied. For example: "A person or an impersonal fact, to which the self is related, is called its object—that of which it is conscious . . . I am always . . . conscious of the private personal object, myself . . . There are certain externalized objects, in particular my body, of which I am so persistently and attentively conscious that I often seem to regard them as part of myself. And, finally, preëminent among the objects of my environment, are the other selves."[57] "The standpoint from which one speaks of objects of the self is, as James says, dualistic. The basis of the conception is the fact that I always find myself conscious of an object: of myself or my experience, of other self or thing or relation . . . Psychology, if it is to take account of the self, must, therefore, take account of the object."[58] If psychology is, then, to study both selves and objects of selves, which may be the selves themselves, we might expect that there should be some differentiation of the two, so that we might know whether we were talking of selves, or of selves as objects of themselves at the moment. But, on the contrary, nowhere in Miss Calkins' psychology are clear distinctions drawn between the self as knower and the self as known. When considering the question of subject and object she writes: "I am always, inattentively or attentively,

[57] *First Book*, 1914, 3 ff.
[58] *Op. cit.*, 280.

conscious of the private, personal object, myself, whatever the other objects of my consciousness . . . It must be pointed out that certain real difficulties attend the classification of the self's objects. There is . . . the difficulty of conceiving the self as both subject and object . . . But these are difficulties only for the metaphysician. The psychologist, who like every scientist, must accept certain facts, without looking for their ultimate explanation, rests . . . on the immediate certainty that I am conscious of myself."[59] After learning that psychology studies both a knower and a known, we are thus told that we should ask no more about them.

If we turn, now, to Miss Calkins' philosophy for the information which she withholds in her psychology, we find that: "In being conscious of a phenomenal fact, the subject (or knowing self) certainly does know an object different from a self. This does not argue against the existence of another sort of knowledge, in which there is no recognition of subject or object—in which, rather, subject and object coalesce in the experience of my consciousness of myself, as knowing and thinking, feeling and willing."[60] However, on returning to the psychology, we discover that: "I always find myself conscious of an object: of myself or my experience . . . . . "[61] In Miss Calkins' philosophy, then, there is no subject-object relation in the "experience of my consciousness of myself as knowing, etc.;" while, in her psychology, there apparently is such a relation.

If we continue reading the "First Book" we become more and more confused; for there the "psychological self" is identified with the subject, or the self as knower.[62] It is, of course, not necessary for psychology to take up the problem of knower and known; but it is evident that, if a psychology continually suggests such a distinction, we should expect its author to state which aspect she is considering. It may also be suggested that the question whether the self is knower or known, or both in coalescence, is just as "scientifically relevant" as the question "Whose idea is this idea?"

Difficulties in the Concept of the Self

It is evident from this discussion of Miss Calkins' self that the concept is an extremely complex one, and one from which it is difficult to pick out the important points. We have seen

[59] *Op. cit.*, 3 ff.
[60] *The Persistent Problems of Philosophy*, 1910, 351 f.
[61] *First Book*, 280.
[62] *Op. cit.*, 281.

that the general concept of the self as a fact, as immediately experienced, as the "plain man's self," is far from clear; that persistence, uniqueness, and complexity can not be distinguishing characters of this self; that, under Miss Calkins' definition of consciousness, relatedness must really include the other three characters and, for that matter, the self itself;[63] and that a consideration of the characters of the self has, after all, done little to clear the concept of the self.

We find, however, still other difficulties in the concept. First, various arbitrary limits seem to be drawn in the study of the relations of the self. For example, we find from the Index of Subjects in the "First Book" that we study the self as "persistent . . . complex . . . unique . . . related . . . as egoistic . . . as altruistic . . . as particularizing and individualizing . . . as generalizing . . . as receptive or passive . . . as active or assertive . . . as perceiving and imagining . . . as recognizing . . . as thinking . . . as emotional . . . as willing . . . as believing . . . as social . . . as religious . . . as object . . . as subject . . . secondary or subconscious self." There seems to be no reason for stopping here. Why should we not go on and discuss the self as singing, as teaching, as philanthropic, as atheistic, as measuring the declination of a star, and so on? For "From the conception of psychology as science of myself in relation to my environment, personal and impersonal, it follows that every concrete personal relation may be the basis of a psychological study."[64] Even with the subject-matter of psychology defined as relations of the self, we are unable to decide what topics it should consider; for Miss Calkins' table confuses rather than clears up the question. The main heading of the table is: "Myself (as subject) is related to (or, conscious of) objects:"[65] This, then, is evidently the subject-matter of self-psychology. But no! for these objects, to which the self is related, are further differentiated into objects of Social Sciences, of Logic, Mathematics, etc., of Physical Science, and of Psychology. Psychology seems here to be made a subdivision of itself, just as, it will be remembered, relatedness was made a subdivision of relatedness.

A more fundamental and a more insistent difficulty is that

[63] If consciousness ("First Book," 1) always "a somebody-being-conscious," then it cannot be true that (*Psych. Rev.*, 13, 1906, 68) "to be conscious is to be conscious of a conscious self." This statement should, on the contrary, read: "to be conscious is to be a conscious self."

[64] *First Book*, 260.

[65] *Op. cit.*, 1914, 4.

the concept of the self is a metaphysical and a logical, not a psychological concept. That it must be a metaphysical concept, if the "plain man's self" is taken as the self, we have already tried to show. That it is a logical concept becomes evident when we find that Miss Calkins is not merely scientifically describing the self, but is rather introducing *meaning* into her concept, is *interpreting* the self. It is the *meaning* of the self that is of prime importance to the plain man; and it is, in the same way, the meaning of the self that is of prime importance to the self-psychologist of Miss Calkins' type.

This criticism has already been made by Pillsbury. "The self, as developed socially, is an interpretation . . . The data that are interpreted we find first, probably, in the constant mass of sensations, strains . . . About these group the socially recognized differences from other individuals, and out of the mass there precipitates an awareness of the self as a meaning."[66] Titchener's criticism of "Der doppelte Standpunkt" pursues a similar line of thought. "With the admission of relational elements into a psychological system the system ceases to be psychological, and becomes so far logical . . . The relational element, in my opinion, is born of the spirit of the older functional associationism, the besetting sin of which lay in its confusion of fact with meaning."[67] The self itself, as well as the relational elements, make the system logical and not psychological; if you start in with *self-meaning* for analysis, as Miss Calkins does, then you must get *relational meanings*. If a "plain man" were asked casually whether or not he considered himself to be persistent, unique, complex, and related, he would possibly agree that he did. He would possibly also agree that he was always changing, was very similar to all other selves, was a unit, and was, in a way, unrelated. In other words, the plain man would accept any meaning which could be applied to his self at some particular time, and would not question the consistency of the characters of the self which he thus obtained. Such a self is, however, not the self directly experienced in those situations in which consciousness of self is very evidently present; and the difference is not merely a difference of degree.

Miss Calkins herself at times acknowledges that she is interested in the logical self; the primary interest of self-psychology is to "understand, not to analyse into elements," that is, to get the meaning, the logical import, not the psychological description. Again we read: "The contemporary functional

[66] *Phil. Rev.*, 16, 1907, 402.
[67] *Phil. Rev.*, 15, 1906, 93.

psychologist, not content with describing consciousness as reaction to environment, commonly lays stress on the 'value,' 'meaning,' or 'utility' of the reaction."[68] But, remember, functional psychology pruned of biological excrescences *is* self-psychology; so, then, self-psychology lays stress on 'value,' 'meaning' and 'utility.'

The Method of Self-psychology

The method peculiar to all psychology is introspection. So far Miss Calkins agrees with structural psychologists. But upon further inquiry it develops that by the term introspection Miss Calkins does not mean 'attention and report under instruction,' as the structural psychologist does, but rather reflection. "To observe myself perceiving, remembering, or judging is no longer simply to perceive, to remember, and to judge, but to reflect upon perception, memory, and judgment."[69] Moreover, she holds that the methods of science in general are to describe exactly and to explain, if possible, the phenomena which are observed.[70] Describing consists of analysing and classifying; therefore, if psychology is a science, we should expect its method to be observation, description, and explanation. We learn, however, that the method used in self-psychology is different from that used in structural psychology; but exactly how it is different, we find it difficult to discover. In "Psychology as Science of Selves"[71] mention is made of the wide divergence in method of the two kinds of psychology; but we learn nothing of the method of self-psychology. Later we are told that every conscious experience may either be "dissected by the structural psychologist and described as a complex of elements"[72] or may be "frankly acknowledged as experience of selves . . . The more closely . . . we observe ourselves perceiving, the more surely we discover in the background of experience the consciousness of other people, actually or conceivably present, who see what we see." How precisely does the method used to find "in the background of experience the consciousness, etc." differ from the method of structural psychology? Again it is asserted that selves are to be "studied in their diverse relations to each

[68] *Journ. of Phil.*, etc., 4, 1907, 681.
[69] *First Book*, 7. While it is true, of course, that reflection precedes and follows the application of scientific method, it is not true that reflection is a method of science. The method of science is observation.
[70] *First Book*, 1.
[71] *Phil. Rev.*, 9, 1900, 494.
[72] *Op. cit.*, 497 ff.

other and to facts of other sorts."[73] And we learn that "a self-being-conscious is not only analysable into elements but is also a complex of relations to its environment."[74] In all this there is nowhere a clear statement of method. In other articles we read that "self is *found to have* [italics not in original] certain characters,"[75] but *how* it is found to have them we can not discover. We read that the merely structural analysis is one of the "several essential analyses,"[76] but we never learn what the other essential analyses are. We read that the description peculiar to self-psychology is a description of consciousness "in terms of the characters of the conscious self."[77] This, to be sure, gives some insight into the *problem* of self-psychology; but, after all, it tells us nothing of the method by which we are to arrive at the description. *How* are we to discover the characters in terms of which consciousness is to be described?

Some suggestion of the method by which we acquire knowledge of the self is given in the sentence: "implicitly, if not explicitly, I am always conscious of a self as having the idea or experience."[78] But what is the state of affairs when we are "implicitly" conscious of the self? According to dictionary definitions, if something is "implicated" or "implied," that something is obtained by a process of reasoning; and if Miss Calkins wishes to employ a term in a sense different from that in ordinary usage, she should define it anew. The specific method of psychology has nothing to do with logical reasoning and logical inference, as Miss Calkins herself affirms when she says: "I immediately experience myself as, etc." So we must leave the "implicit consciousness of self" out of the question; it offers no basis for a psychological system.

We may summarize what we have learned about the method peculiar to self-psychology. (1) The method peculiar to all psychology is introspection; (2) the method of self-psychology is widely divergent from the method of structural psychology: (3) there are other analyses beside structural analyses; (4) every complex experience is both a complex of structural elements and a self related to environment. We must, then, find the method for ourselves. The one difference between the method of self-psychology and that of structural psychology which we find in Miss Calkins' writings is the difference in the

[73] *Op. cit.*, 501.
[74] *Psch. Rev.*, 13, 1906, 63.
[75] *Journ. of Phil.*, etc., 5, 1908, 65 ff.
[76] *Loc. cit.*
[77] *Journ. of Phil.*, etc., 5, 1908, 113.
[78] *First Book*, 274.

meaning given to the term 'introspection.' Miss Calkins uses the term as a synonym for reflection; the structural psychologist uses it as a synonym for observation. The method of self-psychology, therefore, seems to be reflection. This conclusion is sustained by various statements of our author's, such as "The description of imagination as self-consciousness is . . . in exclusively reflective terms."[79] But reflection is the method of logic, and not of science. The conclusion that the method peculiar to self-psychology is the method of logic is, of course, what we should expect if we accept the more general conclusion that the concept of the self is a logical concept, and that the characters of the self have been obtained by logical reasoning.

The Problem of Self-psychology

The problem of self-psychology is never directly stated by Miss Calkins, although we may assume, first, from her discussion of method, that psychology is to describe accurately, to classify and to explain; and from the "First Book," that it hopes to gain "a fuller understanding of these relations [of self to other selves] in their complexity and a deeper acquaintance with my own nature."[80] Moreover, we have self-psychology referred to as "the science of consciousness whose 'primary interest is . . . to understand . . . not to analyse into elements.'"[81] From such statements we must conclude that the problem of self-psychology differs from that of structural psychology in that, besides attempting to describe, it aims also to understand, to obtain a fuller understanding of the relations of selves, and to acquire a deeper acquaintance with one's own nature. But with this problem of understanding and interpreting, self-psychology can scarcely claim a right to the name of science. The problem of a science is to describe accurately the phenomena which it observes;[82] self-psychology has an additional problem, to understand and interpret the relations which it knows immediately or *reflectively*. The opposition of the two is clear.

It is interesting in this connection to continue the quotation

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[79] *Journ. of Phil.*, etc., *op. cit.*, 117.

[80] P. 10.

[81] *Phil. Rev.*, 9, 1900, 494. The phrase 'primary interest is . . . to understand . . . not to analyse into elements' is quoted from Münsterberg's "Psychology and Life." Münsterberg is speaking of history: but in the preface to Miss Calkins' "Introduction to Psychology" we read: "The conception of psychology as a science of related selves is closely affiliated with Münsterberg's conception of history as science of the relations of willing subjects."

[82] *First Book*, 1.

from Münsterberg which Miss Calkins begins. After speaking of the science whose "primary interest is . . . to understand . . . not to analyse into elements," Münsterberg goes on: "We want to interpret their meanings and not calculate their future . . . All we want to know about them is with what other attitudes they agree or disagree. We have the logical aim, to consider them in their relations to all other will-attitudes and to work out the system of these connections." The problem of self-psychology is, then, for the author of the expression "to understand not to analyse into elements," a logical problem. And not only is the problem of understanding and interpreting relations a problem of logic rather than of psychology; but the further problem (not definitely stated, but hinted as an outcome to be expected) of applying the knowledge thus acquired is also a problem for a technology rather than for a science. In the passage quoted above: "A fuller understanding of these relations . . . and a deeper acquaintance with my own nature may surely, therefore, have a significant influence on my behavior."

It is evident that the problem of self-psychology is much broader than the problems of most psychologies. It is one, moreover, that not only transcends the limits of psychology, but that also infringes on the territory of other disciplines, particularly those of logic, epistemology, and the technologies.

The Results of Self-psychology

The results attained by self-psychology are, in their general nature, what we should expect from the subject-matter, method, and problem. Aside from incidental "structural analysis," the "First Book" gives us detailed and careful statements as to the meaning, significance, and use of various experiences. As the method of self-psychology is to describe and interpret in terms of the characters of the self, the results of self-psychology will, of course, be expressed in these terms

Perception is described as "sensational, passive conciousness reflectively realized as impersonal, particularizing, common to other selves, and as related to present external objects." Imagination is described as "sensational, impersonal, and particularizing, but as lacking the consciousness of passivity, the relation to present objects, and the community." The only characteristics of perception, then, which are "immediately realized" are that it is sensational and passive. The passivity of the self is defined as a 'personal attitude' or 'relation of the self to objects.' The statement that the passivity is immediately experienced is contradicted by a statement made elsewhere,[83] in which Miss Calkins grants that there is no "immediately experienced difference between perception and imagination."

[83] *Journ. of Phil.*, etc., *loc. cit.*

Attention is stated to be "a unique attitude, a basal relation of self to object, comparable with receptivity or sympathy or activity. This conception may, but need not, be combined with the teaching that there is a structural element of clearness, or attended-to-ness."[84] Here again we have, as we expected, relations of the self. The expression "basal relation" raises the question of the difference between basal relations and other kinds of relations, and how the two are discovered and differentiated; but these questions are not answered.

Recognition is also discussed as "personal attitude;" whether basal or not, is not said. Under the section entitled "Recognition as Personal Attitude" the essential characters of recognition are formulated; the most important is the "emphasized persistence of the self" (another argument for the theory that relatedness really includes the other characters). Upon structural analysis, recognition is distinguished by the prominence of relational elements. The recognizing self is said to be "(1) relationally conscious of, (2) itself as persistent and of objects as related to its past." It is not clear why this sentence should not have read: "the recognizing self is relationally conscious of itself (1) as persistent and (2) as related to objects in the past, or related to its former self as related to objects." Under such an arrangement, the whole description of consciousness would be in terms of the characters of the self, and the necessity of bringing in "structural" elements would be avoided.

In the chapter on thought: "The thinking self is the self (1) relationally conscious (2) of related objects which (3) it knows, reflectively if not immediately, as objects, also, of other selves." And again, there are relational elements in thought. "Conception is the relational consciousness (reflectively attributed to other selves also) of a group or of an object as member of a group." "By 'judgment' . . . is meant the relational consciousness of a whole as including or excluding certain emphasized features, an experience reflectively known as sharable with other selves."

Emotion is discussed as personal attitude and as affective consciousness. Under personal attitude we find that emotion is, "first and foremost, an intensely individualizing experience. . . . In my emotion . . . I immediately realize myself as a unique self . . . and . . . I individualize the object of my love or hate or fear. . . . A second character is its receptiveness." As affective process, emotion is characterized by relational elements, affective elements, and organic sensations.

"Will" is described as "an egotistically assertive consciousness. It is obviously a profoundly attentive and a doubly individualizing experience. . . . From objects of thought of which one is aware as related primarily to each other, objects of will, like objects of emotion, are sharply distinguished in that they are realized as related to the self . . . and as in a way dependent on the willing. . . . The willing consciousness includes . . . sensations of movement . . . the consciousness of (1) futurity, (2) of connectedness, and (3) of realness—experiences corresponding with the characters . . . of the willed object."

Faith and belief are described as "assertive, doubly individualizing adoptive attitudes to objects of any sort, and as distinguished by the elemental consciousness of realness and by that of congruence." Under the heading "religious consciousness" religion is defined as "the con-

[84] P. 104.

scious relation of human self to divine being, that is, to a being or beings regarded as greater than this human self, or than any of its fellows, and either conceived or treated as personal."

In addition to those sections of the book which deal with descriptions of the various relations of the self in the manner illustrated above, we find other passages entitled: "The Uses . . . of Memorizing," "The Uses and Dangers of Conception," "The Uses and Dangers of Reasoning," "The Significance of Emotion." Here we have, evidently, the fulfillment of the aim to obtain "a deeper acquaintance with my own nature" which "may surely . . . have a significant influence on my behavior."

These results of self-psychology, aside from their general nature and the terms of their expression, are surprising for two reasons. First, since structural psychology is at so great pains to standardize conditions, to repeat experiments many times, and to obtain many observers for every detail of its results, it is amazing to find the results of self-psychology given with such definiteness, with such certainty, when apparently Miss Calkins herself has been the only observer, and we are told nothing of the conditions under which she worked, and are unable to verify her results. In the second place, it is strange that, in a psychology so different from structural psychology as is self-psychology, only the topics familiar from structural psychology are discussed. It will be remembered that, in the Index of Subjects to the "First Book" the self was referred to in many relations; though we might have expected it to be studied in many more relations. Instead, now, of finding the self discussed "as active," and so on, the main topics considered are those which occur in any psychology: perception, imagination, memory and so forth,—instead of the self as perceiving, as singing, and all the rest. However, selection of topics is probably due to the fact that tradition has more or less prescribed the topics for psychology in general.

We pass on to consider the significance of the results obtained. In addition to "structural analysis" we find, first, a description of the varying relations of the self. This description is obviously not the kind that may be obtained immediately from observation. It is, on the contrary, the reflective description which an "arm-chair" psychologist could make as well as, if not better than, a psychologist who attentively observes his actual experiences. Any logician, or any "plain man" who is skillful in drawing distinctions of meaning, could distinguish, for example, between "happiness in another's happiness" and "happiness in another's unhappiness;" be-

tween an "assertive attitude with self as object" and "an assertive attitude with other self as object;" between "perception regarded as shareable" and "imagination regarded as unshareable." But if such distinctions can be drawn by one versed in logic and ignorant of psychology. it is at least possible that logic has been introduced into self-psychology to obtain its results.

The second result of self-psychology is the application of results of the first kind. Application, however, belongs not to science, but to a technology. Physics leaves the application of its facts to engineering and the allied technologies; similarly, psychology should leave its applications to its allied technologies, such (Miss Calkins has herself suggested) as ethics, social science, and pedagogy.

Conclusion

Let us now see whether the claim that psychology is "most naturally, consistently, and effectively treated as a study of conscious selves in relation to other selves and to external objects" has been justified.

In the first place, we must repeat that an evaluation of the system is difficult, by reason of the lack of any method by which we might reach Miss Calkins' results. If she would tell us *how* to get the self, and *how* to get the characters and the relations, how in a word her conditions are to be duplicated, we might then have results of our own which should be comparable with hers. In the absence of any such method, we have found that only by a process of logical inference do we arrive at her self with its characters.

Another difficulty is that, no matter how vigorously many psychologists deny the ever-present consciousness of self (which is the apparent excuse that self-psychology has for being),[85] Miss Calkins insists that these psychologists really are self-psychologists, because they at least imply the self "as basal fact." She writes:[86] "The idea-psychologists are implicitly assuming or leading their readers to assume the existence of a self, when they describe consciousness in such words as 'I attend to a color' . . . and still more when they mark off certain experiences as peculiarly personal, that is, as espec-

[85] Personally, I cannot see why—if we *were* conscious of self all the time—that should necessitate a superior psychology of selves; any more than the fact that we were "inattentively or attentively" conscious of our body all the time should necessitate a superior psychology of bodies.

[86] *First Book*, 279.

ially related to myself."[87] The ground for the assertion that idea-psychologists " unambiguously imply the consciousness of self as part of the experience described in terms of the idea " is apparently the fact that they, like all the rest of the world, use the personal pronoun. The argument, on its most favorable interpretation, is a broken reed. For it is one thing to say that " idea-psychologists . . . assume the existence of a self," and another thing to say that they " imply the consciousness of self as part of the experience . . .." One may " assume the existence of a self " without assuming that one is always conscious of that self; one may assume the existence of the mathematical proof of a formula without being aware of that proof. As to the personal pronoun,—if the use of it makes one a self-psychologist, in Miss Calkins' sense, then her own use of such words as ' garden,' ' motor-car,' ' forehead,' makes her a materialist.

We conclude that Miss Calkins' arguments from " the self as basal fact " are not convincing. We have already suggested (p. 80) that the self of self-psychology seems to be another name for the all-inclusive mental function of functional psychology; and structural psychology can hardly be expected to treat of that self. But, if we cannot accept Miss Calkins' statement that all psychologists are really self-psychologists, can we accept the other statement that " psychology is most naturally, consistently, and effectively treated " as self-psychology? We consider these points in order.

How is psychology most " naturally " treated? How, for that matter, is anything most " naturally " treated? The answer is, apparently that a thing is treated most naturally when treated as the plain man would treat it. There can be no doubt that the plain man would more often consider the relations of persons (or selves, if they must be deprived of body) to each other and to impersonal objects, than he would consider mental processes as such. But then psychology is a science, and not mere common sense. And this puts the matter in a very different light: for no science is " natural " in the sense given above; if it were, it would not differ from casual observation and interpretation; science is, on the contrary, necessarily abstract and, as Miss Calkins would say, artificial.

[87] Miss Calkins is here referring to the statement made by Titchener: " regarded from the point of view of ordinary life, blue and warm are somehow detachable from oneself, whereas pleasantness is always within oneself." But Titchener does not make the statement " from the point of view of psychology; " and the " plain man's self " surely may not be carried to the point of accepting in psychology distinctions which it makes in ' common sense.'

Science does not aim or hope to be either "natural" or non-abstract. If, then, self-psychology is more "natural" than other psychology, in the sense of standing nearer to the view of the plain man, self-psychology loses thereby rather than gains; for the more "natural" or common-sense-like it gets, the less scientific it becomes.

As for consistency: we have shown that the characters of the self are not consistent, and do not fulfill the duty demanded of them (that of differentiating between self, and idea and function); we have shown that the system is not carried to logical completion. Moreover, as we have already suggested, the topics of self-psychology are surprisingly like those of structural psychology. With fundamental conceptions as different as they are, we should naturally expect the two psychologies to deal with different topics. Self-psychology gives us activity, passivity, egoism, and a number of 'personal attitudes' of which we hear little or nothing in structural psychology; and yet we find no new topics, no new chapters. It is a real question whether Miss Calkins has consistently worked out the results which should follow upon her premises; and the answer to the question is, again, bound up with the exhibition of her method.

The third advantage claimed for self-psychology is effectiveness. The meaning of this term, again, is not very clear. But if it means the production of demonstrable results, we may look for such results either within or without the given science. A science, to be effective within its own limits, must provide stimulus and suggestion for further work. Self-psychology, now, sets out to describe and interpret certain relations of the self in terms of the four characters of the self. Let it be granted that the paragraphs of the "First Book" are both complete and consistent; and let it be granted that they could not have been written by a logician, by any one who was not a psychologist: still, only a few of the relations of the self have been discussed, and there is further work to be done if all the relations are to be handled. That is, so far, promising. But Miss Calkins tells us that it is impossible even to enumerate all these relations. So the further work, to which alone, by hypothesis, we are stimulated, is at the outset acknowledged to be hopeless; and we therefore have small enthusiasm to pursue the method of self-psychology further. As for the outside influence of self-psychology, that is to be exerted in "ethics, social science, and pedagogy." But as these disciplines are, at least in a certain stage of their development, older than self-psychology, it remains a question whether they may not

have helped to shape self-psychology as positively as self-psychology has aided or promises to aid them. Moreover, this sort of argument has, as we have seen, another edge; for application places self-psychology among the technologies, and behind technology there must always be science. So far, then, it seems that the claim of effectiveness remains to be clarified and substantiated.

We must conclude, therefore, that, of the three ways in which Miss Calkins claims self-psychology as superior to other forms of psychology, naturalness is an inferiority in a science rather than a superiority; self-psychology is not effective in the sense that it encourages further work; and finally self-psychology has not been made thoroughly consistent. In the second place, if we may judge it by ordinary standards, self-psychology has a subject-matter which is largely logical and metaphysical; employs a method which is reflection, the method of logic; formulates a problem which is a problem of logic and of technology as well as a psychological problem; and furnishes results which might be obtained without psychological training, simply by a process of reasoning.

AN EXPERIMENTAL STUDY OF SENSORY SUGGESTION

By A. S. Edwards[1]

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A. Introduction

Many definitions have been offered of the term Suggestion, and many experiments have been made in whose results the effects of suggestion have found qualitative or quantitative expression. But no one, we believe, has attempted by means of careful introspection to study the suggested consciousness itself. It is matter of common experience that suggestion may arouse, deflect or inhibit movement; it is equally clear that suggestion may change the meaning of a perception or idea by shifting its context, that is, by giving it a new associative setting. But if we ask whether or to what extent suggestion may bring about intrinsic changes in conscious contents, we receive only partial and incidental answer. A continuously rising tone, for example, is presented under the suggestion that the pitch is falling. The observer reports a drop in pitch. Has he, then, heard what he reports, or is he subject to an illusion of judgment?

In connection with sensation, especially, do we find equivocal statements and a lack of experimental study. We find statements that seem to indicate a direct sensory result of sug-

[1] From the Psychological Laboratory of Cornell University.

gestion. Baldwin, in the Dictionary of Philosophy and Psychology, mentions a 'sensory' suggestion; and the illustration given is that of a person who 'sees' a red light as green.[2] But the question remains whether, in such an instance, the observer actually sees green when red is presented; or whether he 'imagines' that he sees green; or whether, after all, the effect is motor, and he makes his reply not as the result of a sensory experience, but simply as a motor discharge which was predetermined by the suggestion. In experiments with suggestion, Scott finds 'illusions' of heat and light;[3] Gilbert reports 'illusions' of weight and sight;[4] Katz writes that a presented blue appeared dark-brown;[5] Seashore maintains that suggested illusions are surprisingly realistic.[6] But in no case do we find a thoroughgoing analysis and methodical control of the processes actually present in the minds of the observers. Külpe writes: "The inference is, perhaps, allowable that in certain circumstances they [centrally aroused sensations] are qualitatively similar to peripherally excited contents." But he concludes that the existing evidence from experiment is unsatisfactory; the experiments that he refers to in this connection, he thinks. "cannot be considered as wholly free from objection."[7]

It is true that hypnotism has been appealed to, in proof of the possibility of producing sensations by suggestion. The hypnotized subject appears to suffer pain, to experience feelings, pleasant or unpleasant, to realize any sensation suggested to him; indeed, Külpe affirms that to suggestion "every organic function, motor and sensory alike, is subject . . . The wildest illusions of sense and the most absurd actions are evoked with equal ease."[8] But sensory results from hypnotized subjects, even if they were fully proved,—as they seem not to be,—could point only to the likelihood, not to the certainty, of similar results in the normal states of consciousness.

Our appeal, then, is to the laboratory and to introspection; and certain factors in the situation must especially be taken

[2] J. M. Baldwin and G. F. Stout, *Dictionary of Philosophy and Psychology*, ii, 1902, 619.

[3] W. D. Scott, Personal Differences in Suggestibility, *Psychol. Bull.*, 7, 1910, 5 ff.

[4] J. A. Gilbert, Mental and Physical Development of School Children, *Yale Stud.*, 2, 1894, 44.

[5] O. Katz, Versuche über den Einfluss der "Gedächtnisfarben" auf die Warnehmungen des Gesichtssinnes, *Zentralb. f. Physiol.*, 20 (16), 1908, 517.

[6] C. E. Seashore, Measurement of Illusions and Hallucinations in Normal Life, *Yale Stud.*, 3, 1895, 62 ff.

[7] O. Külpe, Outlines of Psychology, 1909, 183.

[8] *Ibid.*, 453.

into account. Since there is a natural disposition to give positive answers, it is necessary to prove that the results do not express the mere ripening of motor tendencies. It seems not at all unlikely that a suggestion may set up a definite tendency to report in a certain way, and that the appropriate situation (the critical term in a series of stimuli) is then adequate to touch off the motor response. Again, it is possible that the supposed sensation is in reality an image; the modifying effect of expectant attention must be taken into account. Abnormal conditions, such as exhaustion, morbid emotivity, etc., must, of course, be eliminated.

From this brief introduction we proceed at once to a statement of our special problem, methods and results. A history of suggestion would be blind, and a critical sifting of the outcome of previous experiments on suggestion is impossible, until we gain a positive insight into the nature of the suggested consciousness; these, then, are topics that we reserve for a future paper. Our present results are themselves hardly more than preliminary; the experimental work requires much time, and is often inconclusive; but we hope that other investigators may be attracted to the field, and that data may presently be available that shall justify a physiological theory.

B. Problem of this Study; Method, Apparatus, Observers

Our general problem is that of the nature and extent of the intrinsic changes produced in consciousness by the influence of verbal suggestion. We are especially interested in sensory results, such results as are normally aroused by an adequate sensory stimulus. The experiments are, therefore, planned to favor sensory results. The questions for which we seek an answer are the following: Can verbal suggestion do the work of an adequate sensory stimulus? What percentage of judgments can be inverted, when supraliminal charges of sensory stimuli are used? How far above the limen can we get these inversions of judgment? What are the conscious processes present when such inversions are made?[9]

The experiments fall into three groups: (1) the suggestion group, (2) the control group, and (3) the liminal group.

In the first group, the observers were called upon to discriminate intensities, qualities, etc., under the guidance of a misleading suggestion; for example, a tone rose in pitch when they were told that it would fall. The first trial in every series was made with a minimal difference of stimuli, and the difference was increased in succeeding trials. *E* continued to increase the stimulus-difference until he feared

[9] In regard to the affective processes, we merely raise the question whether the effect of suggestion is not secondary, following upon the arousal of organic sensations. We have no experimental data to offer.

that the observers would detect the deception. Suspicions on the part of the observers were unavoidable; they had to be assured again and again that *E* was giving the stimuli in accordance with his original statement. Whenever there appeared results of suggestion strikingly like the results ordinarily obtained from adequate sensory stimuli, introspections were taken, and the experience was compared with the normal: details will be given later. In the second group, the observers made the same discriminations under the regular methodical instructions. They now made them, however, with full knowledge of the suggestions employed in the first group, and with full assurance that no misleading suggestion would henceforth be used. Further, they were required to give full introspections of the fore and midperiods, with constant reference to the conscious effect of suggestion in the parallel experiments of the earlier group. The third group consisted of determinations of difference limens, by the method of limits, taken in order that the results of suggestion in the first or suggestion group might be subdivided, the trials in which the stimulus-differences were subliminal being set apart from those which gave inversions of judgment when the stimulus-differences were supraliminal. It is this latter set, in which we have what may be called supraliminal inversions of judgment, that interests us in the present paper. *E* took as the difference-limen the first difference of stimuli, correctly judged, which had two correct judgments beyond it. This is a slightly larger limen than is obtained by other forms of procedure; but the larger limen was deemed desirable, since comparison with the results of the suggestion group demanded a margin of safety. In order to make the results of the suggestion and of the liminal groups comparable, the steps in the corresponding series were kept the same. The liminal series served also to assure *E* of the normal acuity of the observers for the sense departments investigated.

Experiments 7, 8, and 9 were complete without the control group. In the remaining experiments, the control followed the suggestion group in inverse order of series. If this arrangement made the burden of memory greater for some, it greatly relieved it for other experiments; and we considered it as, on the whole, the best plan to adopt. The control for experiment 3 came immediately after the parallel suggestion experiment; for 11, there was an interval of a week; for 2 and 3, four and five weeks (3 months for one observer); for 10, two months; for 1, three months; and for 6, one, four and five months for the various observers.

Experiments were made—not all, as we shall see, with the same thoroughness or success—upon sight (quality, intensity, extent), hearing (noise intensity, tonal quality and intensity), taste (quality), smell (quality, intensity and duration), pressure (intensity), and temperature (intensity of warmth and cold). There were four experiments upon sight; three upon hearing; one upon smell; one upon taste and smell; and one each upon pressure and temperature. Most of the experiments were planned for inversions of judgment; in two cases, however,—one of the audition experiments and the smell experiment,—the attempt was made to get, by means of verbal suggestion, the process ordinarily aroused by adequate stimulus, and to get it in the absence of any adequate sensory stimulation whatever. In three cases, owing to the nature of the experiment, to lack of time, or to difficulty with apparatus, we failed to obtain liminal determinations.

For vision, colored, black and white discs were mounted on the Marbe mixer;[10] recourse was also had to Whipple's apparatus for the discrimination of brightness.[11] For visual extent, we used Wundt's space-limen apparatus.[12] The Stern variator, Fechner pendulum and modified Lechmann acoumeter[13] were employed for sound experiments. Two paper funnels were used in the smell experiments; small candies for taste and smell; the Wundt pressure apparatus[14] for pressure, and brass cylinders for temperature. Continuous changes were possible with the Marbe mixer, the Whipple box, and the Stern variator.

In experiments of the first and second groups (with exceptions noted later), the observers were told, either that a stimulus would be given and that shortly afterwards a second would follow, or that a continuously changing stimulus would be given; they were to judge the direction of the change and, when asked, to give full introspections; the suggestion as to direction of change was made just before each trial. The suggestion always ran counter to the facts; so that if *E* said the change would be toward brighter, he changed the stimulus toward the darker. The observers were informed that the first changes would be small, and that the series would be continued until they could clearly perceive the change suggested. In the experiment on smell, the observers were told that they would be given an odor and that, as soon as they indicated their perception of it, a second odor would be given which should neutralize the first. No odors were used, and no experiment was begun until the observer reported the room free from all odors. In the experiment on taste and smell, the candies were given in pairs, the one to be eaten shortly after the other; the two differed in shape, and one was so wrapped as to appear of better grade than the other. The ingredients were the same in both cases. The suggestion lay in the appearance of the candies and in the verbal instructions; the observers were to write full introspective comparisons of the sensations of taste and smell aroused by the two kinds. In one of the audition experiments, a lead and a cotton ball were used, the sight of the falling cotton ball being the suggestion for the sound. The object of these experiments was, then, either to arouse through suggestion the process commonly aroused by an adequate stimulus, or by means of suggestion to invert judgment. The omission of several steps at the end of a series permitted the experimenter to obtain, in a few instances, very large supraliminal results, and at the same time to avoid the danger of detection, which was always present, but was especially to be feared in the longer series.

The experiments were carried on with five men in the Cornell laboratory during the greater part of two years, and later

[10] K. Marbe, *Physiol. Centralb.*, 1894, No. 25, 811.
[11] G. M. Whipple, A Manual of Mental and Physical Tests, 1910, 163.
[12] W. Wundt, Physiol. Psychologie, iii, 1911, 466.
[13] E. B. Titchener, Exp. Psych., II., 1, 1905, 24.
[14] W. Wundt, *op. cit.*, i, 1908, 674.

with five women at the University of Minnesota during one year. The men will be referred to as *A, B, C, D* and *E;* the women as *F, G, H, I* and *J*. Observers *A, B* and *C* had all had thorough training in introspection, *A* and *B* in the Cornell laboratory and *C* in the Harvard laboratory. One was an instructor in the department, one an assistant, and the third an advanced student engaged in research. *D* and *E* were respectively a fellow and a scholar in psychology. *F, G, H, I* and *J* were advanced students in psychology, one being an assistant in the department with some training in introspection; all needed further training.

A number of trials were made with other than the regular observers. These did not call for introspective reports; they were undertaken merely for comparative purposes. They showed that results similar to those obtained from the regular observers could with little difficulty be obtained from persons unconnected with the psychological laboratory and unversed in psychology. Indeed, in some instances, even more extreme results of suggestion appeared with these untrained observers.

In reporting our experiments, we shall first give the quantitative results which show the effects of suggestion in relation to the difference-limen. We shall then turn to an analytical and qualitative study of the conscious processes under the influence of suggestion.

C. Quantitative Results

In calling these results 'quantitative,' we do not mean that we have measured the degree of suggestion, or that we have discovered the greatest amount of supraliminal change of stimulus that can be introduced while the judgment of the observer under the influence of suggestion is still inverted. We are only reporting, in numerical terms, the results of particular series of experiments made under particular conditions. These conditions were, in fact, distinctly unfavorable to suggestion. The instructions demanded a critical attitude; the observers were asked to make judgments in series which, so far as they knew, were part of a study of liminal differences, and which demanded their most careful attention. Seven of the ten observers reported suspicions as to the nature of the experiment, and all showed, at times, by action and tone of voice, that they thought the investigation was not what it purported to be. Finally, all such factors as fatigue, excitement, and the like, which are favorable to suggestion, were absent.

The difficulties of the experiments, the suspicions of the observers, and lack of time, made it possible for *E* to use supraliminal charges of stimuli only in 900 of the 1621 trials comprised in the first or suggestion group. In 253 of these

900 trials, the suggestion succeeded in inverting judgment. In other words, suggestion was successful in 28 per cent. of all cases in which supraliminal changes of stimuli were used. This number does not include the judgments 'doubtful' and 'equal.' Of the 254 series in the group suggestion was successful in 110, or in 43 per cent. The following Tables 1, 2 and 3, show (1) the percentage of successes of suggestion with supraliminal changes of stimuli for the various series in the suggestion group, and (2) for the individual experiments and observers, and (3) the number of steps above the difference-limen where suggestion was successful, with the number of the successful cases for every step. To repeat and emphasize, all the results noted in these Tables are results obtained with supraliminal differences of stimuli.

TABLE 1

Number and percentage of series from the suggestion group containing inversions of judgment.

Part 1
According to Experiments

Expt.	Suggestion	No.	%
1	Blue.....	17	71
3	Brighter.....	21	72
	Darker.....	20	74
4	Larger.....	0	0
	Smaller.....	2	20
5	Louder.....	4	80
	Fainter.....	4	80
6	Higher.....	9	45
	Lower.....	..	..
7	Falling shot...	14	16
10	Heavier.....	7	100
	Lighter.....	0	0
11	Warmer.....	3	36

Part 2
According to Observers

Observer	Series	%
A	10	25
B	15	34
C	35	42
D	3	100
E	4	100
F	14	86
G	6	35
H	3	50
I	10	52
J	10	48

Total series containing inversions = 110 or 43%.

In Part 1, the first column indicates the number of the experiment; the words Blue, Brighter, etc., the suggestion used. The numbers following show first the number (no.) and secondly the percentage (%) of series in which inversions of judgment followed the use of suggestion. E. G., "17 71" means that in Experiment 1 inverted judgments were found in 17 series, or in 71% of all series in the suggestion group.

Part 2 indicates the number and percentage of series containing inversions of judgment for each observer.

TABLE II

Number and Percentage of Supraliminal Inversions

Exp.	Suggn.	A	B	C	F	G	H	I	J	Total errors	Av. %
1	Blue	(3 15)	(6 75)	(37 86)	27 44	1 3	11 17	16 76	11 22	66	28
		A	B	C	F	G	H	I	J		
3	Brighter	4 44	3 15	6 25	18 75	3 15		8 37	9 50	51	37
	Darker	0 0	7 33	8 20	5 50	3 16		17 71	6 23	46	32
		A	B	C							
4	Larger	0 0	0 0	0 0						0	0
	Smaller	4 36	0 0	4 23						8	21
		A	B	C							
5	Louder	1 25	3 14	9 69						13	51
	Fainter	1 17	2 18	10 77						13	43
		A	B	C							
6	Higher	1 50	0 0	5 78						6	35
	Lower	0 0	1 25	8 25						9	38
		A	B	C	F	G	H				
7	Shot	2 6	3 13	7 13	2 20	0 0	0 0			14	10
			D	E							
10	Heavier		4 40	4 100						8	57
		A	B	C							
11	Warmer	2 66	1 10	4 66						7	35
	Colder	3 75	3 16	6 100						12	43

	A	B	C	D	E	F	G	H	I	J
Inversions per observer	18	23	67	4	4	52	7	11	41	26
Percentage " "	20	16	27	40	100	49	8	15	60	27

Total errors for all observers = 253 or 28%

Numbers in the first column refer to experiments 1, 3, 4, etc. The words Blue, Brighter, etc., refer to the suggestions used. The figures in the body of the table show the number and percentage of supraliminal inversions. For example, under A are found the numbers 3 15; these mean 3 errors, which were 15% of the possible errors.

The totals and average percentages do not include results for A, B and C in Exp. 1, since limens were not determined. If the limens for A, B and C are not higher than the highest limen for F, G, H, I and J in this experiment, the bracketed figures are correct.

For example, under A are found the numbers 3 15; these mean 3 errors, which were 15% of the possible errors.

The totals and average percentages do not include results for A, B and C in Exp. I, since limens were not determined. If the limens for A, B and C are not higher than the highest limen for F, G, H, I and J in this experiment, the bracketed figures are correct.

TABLE III

NUMBER OF INVERSIONS FOR EACH SUPRALIMINAL STEP

Experiment 1. Suggestion: "Towards Blue"

	Degrees:	6	8	10	12	14	16	18	20	22	24	26	28	30	32	34	36	38	40	42
A	DL			1					1		1									
B	DL		1	1	1	1	1	1												
C	DL	2	1	1	3	3	2	1	2	2	1	2	2	1	1	1				
F	DL 5.1, m.v. 1.5		2	4	2	2	2	2	1		1		1	2	1	1	1		1	1
G	DL 7.9, m.v. 1.2						1													
H	DL 7.0, m.v. 1.8		2	2	2	2	1	1	1											
I	DL 10.5, m.v. 1.1				3	2	1	3	3	2	2					1				
J	DL 7.0, m.v. 1.2			1		1	2			2			1	2	1	1				

Experiment 3. Suggestion: "Towards Brighter"

	Degrees:	4	6	8	10	12	14	16	18	20	22	24
A	LDL 9.2, m.v. 3.1				1	1						
B	LDL 3.7, m.v. 1.5	2	2	1	1	1						
C	LDL 2.4, m.v. 1.0	1	2	1	1	1						
F	LDL 7.6, m.v. 1.2			3	4	2	2	3	2	2		
G	LDL 5.8, m.v. 0.4				1		1					1
H	LDL			2	2	2	1	2	1	2	1	1
I	LDL 7.7, m.v. 1.1			2			2	1	3	2		
J	LDL 6.1, m.v. 1.9			1	3	1	1	1	1	1		

Experiment 3. Suggestion: "Towards Darker"

	Degrees:	4	6	8	10	12	14	16	18	20	22	24
A	UDL 3.7, m.v. 1.7		1	1								
B	UDL 5.3, m.v. 1.0	1		1		1						
C	UDL 2.5, m.v. 1.2	2	1	3	2							
F	UDL 6.8, m.v. 1.8			1	1	1	2					
G	UDL 7.9, m.v. 1.4						1					1
H	UDL			2	2	2	2	2	2	2	1	
I	UDL 8.6, m.v. 1.7				1	3	3	2	4	3	1	
J	UDL 6.6, m.v. 2.2			1	1			2		1		

TABLE III.—*Continued*

		Experiment 4. Suggestion: "Towards Larger"							
	Millimeters:	1	2	3	4	5	6	7	8
A	LDL 2.0, m.v. 0.7								
B	LDL 2.0, m.v. 0.7								
C	LDL 3.3, m.v. 0.5	many "equal" and "doubtful" judgments							
		Experiment 4. Suggestion: "Towards Smaller"							
	Millimeters:	1	2	3	4	5	6	7	8
A	UDL 1.9, m.v. 0.9		2	1	1				
B	UDL 3.4, m.v. 0.5								
C	UDL 2.8, m.v. 1.2	many "equal" and "doubtful" judgments							
		Experiment 5. Suggestion: "Towards Louder"							
	Divisions on scale:	1	2	3	4	5	6	7	8
A	LDL 1.6, m.v. 0.8						1		
B	LDL 3.3, m.v. 0.8				1	1	1		
C	LDL 2.7, m.v. 1.8			2	2		2	2	1
		Experiment 5. Suggestion: "Towards Fainter"							
	Divisions on scale:	1	2	3	4	5	6	7	8
A	UDL 2.6, m.v. 1.3						1		
B	UDL 2.7, m.v. 1.1								
C	UDL 3.3, m.v. 1.6				2	2	2	1	1
		Experiment 6. Suggestion: "Towards Higher"							
	Vibrations:	5	6	7	8	9	10		
A	LDL 4.5, m.v. 0.7	1							
B	LDL 6.9, m.v. 1.3								
C	LDL 5.3, m.v. 1.1		3		2				
		Experiment 6. Suggestion: "Towards Lower"							
	Vibrations:	5	6	7	8	9	10		
A	UDL 4.2, m.v. 1.7								
B	UDL 5.3, m.v. 0.6						1		
C	UDL 5.5, m.v. 1.5		6		2				

B	UDL 5.3, m.v. 0.6.....					1	
C	UDL 5.5, m.v. 1.5.....		6		2		

TABLE III.—*Concluded*

Experiment 10. Suggestion: "Towards Heavier"

	Grams:	6	8	10	12	14	16
D	LDL 11.0, m.v. 3.4.....						
E	LDL 10.2, m.v. 1.9.....						

Experiment 10. Suggestion: "Towards Lighter"

	Grams:	6	8	10	12	14	16
D	UDL 4.8, m.v. 1.8.....	2	1		1		
E	UDL 8.4, m.v. 1.8.....		2	2			

Experiment 11. Suggestion: "Towards Warmer"

	Degrees:	1	2	3	4	5	6
A	LDL 2.1, m.v. 0.7.....			1	1		
B	LDL 0.9, m.v. 0.3.....		1				
C	LDL 0.7, m.v. 0.3.....	3	3				

Experiment 11. Suggestion: "Towards Colder"

	Degrees:	1	2	3	4	5	6
A	UDL 2.0, m.v. 0.5.....	1	1				1
B	UDL 3.2, m.v. 1.8.....		1	2			
C	UDL 0.7, m.v. 0.3.....	3	3				

Table 3 shows the size of the steps, i.e., the amount of supraliminal change of stimulus, for which judgment was inverted under the influence of suggestion. Below the statement of the suggestion are given the various amounts of change of stimulus from the zero point; these are given in degrees, millimeters, etc. The first column indicates the observers; following this are given the difference-limens and their mean variations; the body of the table shows the number of cases of inverted judgment for every supraliminal step used.

D. Qualitative Analysis of Consciousness under the Influence of Suggestion

Experiment 1. Color

The Marbe Mixer was used for the introduction of yellow by continuous change into red. The change in the first exposure of the series was from 0° to 2°, in the second from 0° to 4°, and so on, the increase in yellow proceeding by steps of 2°. Each series was as long as *E* could make it without running an obvious risk of detection. A large gray cardboard served as a screen. The observers seated themselves at such a distance from the discs as made observation easiest for them; the distance was about 3m. The experiments were made with only one observer at a time, for periods of 50 minutes. Exposures were of 6 secs. duration, increased slightly for the trials where the greatest amounts of yellow were introduced, in order that the rate of introduction might be roughly the same. Instructions were as follows: "I am going to run a blue into a red on the color mixer. The change will be continuous. I want you to tell me whatever changes you notice in the color. The first changes will be very small, the changes in succeeding exposures increasing until you are sure of the change." The suggestion was generally, but not always, repeated just before each exposure: "Towards the blue," or "Still towards the blue."

Suggestion Group

Observer *A* reports as follows with the following degrees of yellow present: 10°, "Barely possible it is darker;" 20°, "Perhaps slightly darker;" 22°, "Not at all certain whether the disc is a little bluish or not now. Quite certain of change but not sure of direction of change. Is it toward blue?"; 30°, "I suspect all of a sudden that maybe there is blue in it and I've missed it before;" 36°, "Slight edge of color of rather greenish gray." *A* reported the following "prepossession": "*E* is working on suggestion, may give me no color at all, may give me wrong colors, may give me no changes, or wrong changes. May make movements intended to deceive me, also make time intervals wrong for deceit;" "Tendency to be careful;" tendency to make statements with "qualification, 'perhaps.'"

Observer *B* reports gray film, and says that it "may be due to the running in of gray." In the second of the two series *B* says, with 8° of yellow, "I guess there is a little blue in it;" and calls 6, 7, and 8 (12°, 14°, and 16°) the same as 4 (8° of yellow).

Among the reports of blue by observer *C* occur the following: 8° of yellow, "Blue ring perceived and decided it was no illusion;" 12°, "Blue ring definitely present;" 28°, "Blue ring definitely present;" 32°, "Definite blue ring not so vivid as 14 [28° of yellow] but more

than 15 [30°];" 32°, "Blue ring as faint as 15." From another series: 12° of yellow, "Obviously bluer;" 22°, "Much bluer than 1;" 28°, "Color has blue in it." After this series *C* was asked to indicate with discs how much blue he thought had been used, and he set them with 35° of blue exposed.

Observer *F* gave the following reports: with 18° of yellow, "Slightly more blue than 5 and 6;" 10°, "Noticeably more blue;" 12°, "Same as 5; perception of blue was less marked in appearance but lasted for a longer time;" 28°, "Perception of red changed distinctly towards blue; . . . surprised to see the blue so distinctly. . . . The blue present was very definite;" 34°, "Blue was very distinct, so much so that the color was quite blue;" 36°, "Blue lasted longer than in 15;" 42°, "The red seemed to be slightly darker, with a gray more than a blue."

Observer *G* was frankly suspicious after a few trials; her brother, she said, had 'fooled' her so often that she was generally on her guard. Some results were obtained: 8° of yellow, "Doubtful; first judgment 'darker,' second judgment 'lighter';" 14°, "Two judgments; one that the hue grew darker; second judgment 'no change';" 16°, "Changes from darker to lighter and darker again." *G* reported 'no change' or 'doubtful' for changes of 8°, 10°, 12°, 16°, 22°, 24°, 26°, 28° and 30°. At 28° she said: "Might have been a change towards blue."

Observer *H* reported the following: 10° of yellow, "Very faint blotches of slightly darker hue;" 10°, "Darker hue seemed to be in narrow bands;" 10°, "Slightly darker;" 10°, "Darker hued;" 18°, "A bluish-pink tinge in a filmy form, seeming to stand out from the background." Many reports of "No change" were made with the yellow shown at 10° to 40°.

In a few cases, *E* reversed the direction of stimulus-change at the end of a series, in order to avert a suspicion which had become evident in the observer's voice, manner or replies. In one series with *H* the results show that within a few trials a large increase of yellow and a large decrease of yellow can both be judged as changes in the same direction. In another series *H* reported "No change" when the stimulus-change was an increase of 34° and 40° yellow (14 and 17 steps beyond the DL).

Observer *I* frequently reported blue: 14° of yellow, "Medium blue discs . . . blue shadows;" 16°, "Single grayish blue disc rises over red from below and remains, not quite covering it;" 20°, "Red seems to blend with blue-gray;" 14°, "Bluish shadow appeared about a second after disc was exposed, . . . moved back and forth;" 18°, "Distinctly a violet-blue film over surface;" 20°, "Change to a purplish red with a great deal of gray in it, and this darker red, not a purple but a purplish red, . . . seemed to fuse this time except at edges and right in center of disc;" 22°, "Yes, grayish blue, . . . a little denser and a little darker and more mixed with the red than it had been before;" 22°, "Change to blue. It was unquestionably blue. . . . The blue was like a veil over the red, but it was unquestionably blue. It did not fuse with the red." Other reports of like nature were made.

Observer *J* reported the following: 10° of yellow, "Part . . . seemed a little darker . . . ;" 28°, "Columns of shadow . . . ;" 22°, "Columns of darkness;" 14°, "the orange-red changed to a brownish-red;" 30°, "Perception of a bluish-lavender, . . . perception of a more brownish-red backgroumd; the bluish-

lavender gradually changed to a more distinctly dark blue perception. The dark blue was not very definite, although I was certain that it was blue and no other color;" 32°, "A bluish-lavender appeared, . . . I am certain that the perception was that of bluish-lavender;" 36°, "No change."

Further results of the same kind might be quoted from the reports of all observers. Needless to say, pauses were often made in the longer series, to avoid fatigue and eye-strain; the observers were continually cautioned beforehand not to make an observation unless entirely free from after-images; and the experimenter frequently enquired, during an experiment, about the presence of after-images.

Control Group

Introspections were secured from observers *F, G, H, I* and *J*. We can give only brief summaries.

Observer *F* rarely reports imaginal processes; judgment comes "following directly the perception of the blue," "with the perception of blue as the clearest thing in consciousness." Once she reports a "readiness to say blue." There is apparently no change of attitude from the one group to the other; and assurance is no greater in the control than in the suggestion series.

Observer *G* expresses her judgments in "Yes," or "Positive," but a good deal of doubt is still reported. There is sometimes much imagery, expectant and memorial, and sometimes again this imagery precedes judgment; but ordinarily the judgment is reported as direct. There is change of attitude, but no marked increase of assurance.

Observer *H* has a good deal of imagery, and sometimes reports verbal processes, though both alike are usually "very dim," "merely an undercurrent of little importance." In recalling earlier series *H* says: "The changes to blue . . . in the suggestion series were clear perceptions, though not of so great change. . . . They were perceptions from the fact that what I saw was different from what I expected to see, i. e., a dark sector on red."

The shift from a suggestion series to a control series was made with observer *I* during one of the hour-periods. It was not, however, until 18° of blue were introduced that she said "Blue was not so blue in the other series." Her introspections show two kinds of judgment: in the one, there were many imaginal processes, in the other no imaginal processes at all were reported, and the judgment was touched off directly by the stimulus. Her assurance is the same in both groups. She used, in the first group, such words as "unquestionably," "distinctly," "clearly," in referring to changes toward blue, and showed no trace of doubt in tone of voice or in word. "As to the earlier group there was no question of the clearness of change and my sureness of the change. It was distinct, and a change, so far as I can tell, actually of sensation . . . ; the changes had all the characteristics of sensory experiences, and, at the time, I should not have thought of calling them anything else than sensory."

J's judgments occasionally come with a "yes," and kinaesthetic-verbal elements are characteristic of her reports. In comparing the two groups she says: "No more clear or distinct than in the cases where I was sure of the blue;" "Experiences exactly like those in other experiments; things seem to go just the same."

Summary of Results from Experiment 1

(1) The records of the suggestion group show results that are very similar to (in some cases indistinguishable from) results reported in the control group. There are a few of these for A, B and G; many for C, F, H, I and J. Even where the observers think that they have found specific differences in the experiences of the two groups (as happened in certain instances with *I* and *J*), reference to the record shows that exactly the same things are reported in both sets of experiments.

(2) The assurance of *F*, *H*, *I* and *J* is the same for both groups; that of *G* is not markedly different.

(3) The introspections show (*a*) judgments with a good deal of imagery; (*b*) judgments which seem to indicate the touching-off of a tendency to agree with the suggestion, to say "Yes" or the like; and (*c*) direct judgments without imagery and with no apparent intervention of the motor impulse to accept the suggestion; here the observation itself appears to be modified. Judgments of the third or sensory type were largely predominant in this experiment with observers *F* and *H*, and were in the majority with *G* and *I*.

(4) The reversal of direction of large stimulus-changes, for two observers, is in the one case barely noticed and in the other, is accepted as a continuation of the series. Definitely supraliminal changes in opposite directions are, by both observers, judged as changes in the same direction.

Experiment 2. Colorless Discs

This experiment, made with black and white discs on the Marbe mixer, seemed fatally to arouse (or confirm) the suspicions of the observers. Hence *E* took only one series with *A* and five with *C*. In the series with *A*, the suggestion was "Towards brighter." With 6°, 8°, and 10° decrease of white *A* reported no change; with 12° he said: "Might have increased that time; can't be sure at all." The obvious suspicion of *A* made it necessary to discontinue the experiment. Observer *C* gave the judgment "equal," under the same suggestion, with decreases of 8° and 10°.

Experiment 3. Colorless Light

The Whipple box, set up in the dark room, was arranged to show light only in one window. The bar connected with the reflector was lengthened, so that small changes of stimulus were possible. Steps of 2° on either side of the neutral position of the reflector were used. The instructions were: "I shall give you a continuously changing light, and I want you to report as soon as you notice the change in brightness. Please indicate also the direction of the change. The change in the first trial will be very small, and will be increased in each succeeding trial until you notice the change." The suggestion

was repeated at the beginning of every series, and generally before every trial: "Towards the brighter," or "Towards the darker." Fifteen minutes were allowed for dark adaptation, and care was taken throughout the experiments to avoid after-images. The exposure was of 6 secs. duration, and was slightly increased for the larger changes.

Suggestion Group

Observer *A*, with the suggestion, "Towards brighter" and the stimulus-change actually from 0° to 12° darker, reports: "Believe it did get lighter right near end." Again, with 12° change towards darker: "A wee mite lighter and then a wee mite darker, very small but *very definite*." With 6° and 8° darker *A* reported "lighter, doubtful;" and with 10° darker, "Wee mite lighter." It is to be noted that, just before this series, *A* had asked *E* to give a few practice trials in each direction, and that *E* had granted the request.

B's reports frequently raise the question whether the change is as suggested. With 8°, 10° and 12° change in both directions, "no change" and "doubtful" are reported. *B* was evidently suspicious; and in the last series, where the suggestion was "Towards darker," *E* reversed his direction. The change was from 8° in the one direction to 12° in the opposite direction, a change of 20°; it was, however, accepted simply as a continuation of the series.

Observer *C* gives many "doubtful" and "no change" judgments, even where the stimulus-changes are clearly supraliminal. But he also gives inversions of judgment with full assurance. With the stimuli 8° and 10° lighter he reports "darker" emphatically. Again, with the stimulus 12° darker he says: "Brighter, sure." Once, when the stimulus-change was 12° darker, he reports: "Three periodic changes constituting increase of brightness, each followed by a darkening. That actually occurred subjectively."

Observer *F*, with suggestion "Towards brighter" and stimulus-change 20° darker, says: "Got brighter, distinctly brighter, then darker. . . . Change from lighter to darker was very slight." In another series, with suggestion "Towards brighter" and stimuli 10°, 12°, 14°, 16°, 18° and 20° darker, all judgments are "brighter." With 18°, e. g., *F* says: "There was a change from darker to brighter, quite distinct, short duration;" with 20°: "Much brighter than last, grew brighter for a time, and then a dark streak through center." Just before the second series of this experiment *F* asked: "Isn't there a good deal of suggestion here?" and added, "My natural tendency is to do the opposite." Immediately afterwards, with the suggestion "Towards darker" and actual changes of 8°, 12° and 14° towards the lighter, *F* reported "darker," and said in every case that she was sure of the change.

Observer *G* was still on her guard. When the judgments were inverted, she was generally doubtful. With a stimulus-change 22° brighter, she reports: "That seemed darker; quite surprised at judgment; not very sure of it; seemed to get lighter at end; then more sure of judgment after I looked at it again; seemed an evident change." In another series, with a stimulus-change 18° brighter, she says: "Seemed as though getting a little darker; not sure; two or three judgments; first that it was darker; then that it was not darker; then that it was darker. Think it was imagination more than anything else."

Observer *H* gets definite results with full assurance. With suggestion "Towards darker" and stimulus-changes of 10°, 12°, 14°, 16°, 18°, 20° and 22° towards lighter, all in one series, *H* reports that the changes are towards darker. In another series, "Brighter" is reported when the stimulus-changes are 6°, 8°, 10°, 12°, 14°, 16° (18°, "no change"), 20°, 22°, 24° and 26° darker.

The reports of *I* also show definite results. Characteristic is the following: 18° darker, "Change half way through; seemed to light up and a difference in depth; seemed to have third dimension, depth, as if I could put my finger into it; then stayed at this brighter intensity the rest of the time." Once when the stimulus-change is 18° brighter *I* reports changes in both directions, but says she is surer of the change towards darker. It is to be noted that this observer often reports twofold changes of this kind, and that she is always surer of the suggested changes than of those due to the actual change of stimulus. *I* even uses the word "brilliant" in describing a change with the stimulus 20° darker.

Observer *J*, with suggestion "Towards darker" and stimulus-change 10° lighter, reports: "Noticeable change first towards darker; constant for a while; then slightly brighter; change towards darker came suddenly, was distinct but only a slight change; change towards lighter was not quite as distinct; I was not as sure of that." With a change of 22° towards lighter she reports: "Distinctly towards darker; very distinctly noticeable; greater change than in any of the other experiments."

Control Group

Observer *A* reports an "agreement" which he "seems to have ahead of time," and he often replies with a "Yes;" "Judgment shot itself off, 'Yes.'" Sometimes *A* also reports a good deal of imagery. He is, however, an observer of a rather extremely 'objective' type, with a persistent tendency to hold fast to the stimulus and to accept the sense-impression as it comes. His judgments, so far as we can tell, are nearly always of the sensory type. There seems to be no greater assurance in this group than in the suggestion group.

Observer *B* often replies by "yes," and reports much kinaesthesis (eye-strain, tendency to nod head, etc.). Once he reports: "A vague memory of previous experiments when I had to be on my guard." His attitude and assurance were changed in the control group.

Observer *C* reports changes no greater or more definite in this than in the suggestion group. In fact we find an emphasis placed on a few, at least, of the inverted judgments which we do not find in the control group. *C* gives many imaginal and mixed judgments; but the sensory are the more numerous.

Observer *F* reports judgments with and without imagery. When asked to compare the experiences of this group with those of the suggestion group she said: "At the time they seemed to be actually sensations. There did not seem to be anything imaginal about them. I should probably have declared under oath that there was a change 'towards brighter' or whatever you suggested." After another trial she said: "Same effect though not as distinct as last time. This was about the same as in the first group. I don't think that if I had the two side by side I could have told any difference, unless this latter might be a little clearer and I can't be sure of that. They were very much alike, I am sure of that." And again: "In trying to decide whether my experiences were sensory or imaginal, there seemed to be both sensations and images, but I seemed to get things I did not expect

and had not imagined. Sometimes the light seemed to be brighter, so much so that I felt surprised. . . . Some of the results were different from that imagined, i. e., expected. It seems to me now that suggestion affected both imagery and sensations, but I can't tell which was affected most."

Observer *G* expresses much doubt in this group, but less than in the suggestion group. With 20° change towards brighter she is doubtful, and with 24° she says: "Guess that was brighter, not sure." After an observation with 20° darker, she remarks: "This seemed about the same as results in the suggestion group. I don't seem to notice any difference between this last experiment and the suggested experiences." Later, in support of her statement that her former judgments were due to 'imagination,' she says: "In sensation I see the change towards darker as a cloud. If it were imagination, it would be confined more to the window itself. When I shut my eyes I can get a clear-cut image of the window itself." But several reports in the suggestion-group refer to just this "cloud"-effect, which the observer maintains would make the experience sensory.

Observer *H* can find no difference in the experiences of the two groups. "Can't now pick out any differences. Changes seemed definite then and they are definite now." "The changes in the first group seemed just as much out in front of me; these in the second group no more clear or definite. . . . I think the change was sensory because the change came differently than I expected it would; I had images of what I expected, and what I saw was contradictory to the images." It should be noted that in this case the control group followed the suggestion group at an interval of only a few days.

Observer *I* agrees with *H*. "At the time I looked at them [the exposures of the first group] they were perfectly real; they looked just like a moving picture or panorama that moved in front of me; it was different from imagination and I did not doubt its objectivity." 'They seemed like sensory experiences in that they seemed to be presented to the eye with visible changes. . . . There was also an . . . idea that the changes were not quite what I expected them to be; it was a little feeling of dissatisfaction, although I had no definite images of what I should get. . . . If you had said to me that it was just my imagination, I should have taken exception to it, because I really saw something there."

Observer *J* has slightly less assurance here than she had in the suggestion group. She reports that there is no difference that she can name between the experiences of the suggestion and of the control groups; "The effect of suggestion in earlier group was certainly, I think, on the sensations."

Summary of Results from Experiment 3

(1) Again, similar (and in part indistinguishable) results are obtained from the experiments of the two groups.

(2) The assurance of *A, C, F, H, I,* and *J* is approximately the same in both groups. *I* expresses greater confidence in the suggested than in the actual change, when on certain occasions she reports change in both directions.

(3) The introspections again show three types of judgment, the imaginal, the motor, and the direct sensory judgment. The sensory type is the most frequent in this experiment: the judgments of ob-

servers *A*, *H*, *I* and *J* are predominantly direct; those of *C*, *F* and *G* are sensory or imaginal or mixed; those of *B* are largely motor.

(4) *A* finds a "very definite" change (judgment being inverted) immediately after *E* has given, at *A*'s request, special practice trials with knowledge. *B*, in the suggestion group, accepts a very large change of direction as simply a continuation of the series.

Experiment 4. Visual Extent

The Wundt apparatus was arranged to show a white rectangle, 14 by 60 mm., upon a black background. The rectangle could be lengthened or shortened at will; the unit of change was 1 mm. The observer saw the standard strip for 1 sec.; then came a pause of 5 secs.; and then the comparison strip for 1 sec. The instructions were: "I shall expose a rectangle, first, at the standard size; then I shall expose it at a length either greater or less than the standard. You are to note any change in length, and to indicate whether the second rectangle is longer or shorter than the standard. The change in the first pair will be very small, and will be increased in succeeding pairs till you notice the change." The suggestion was repeated before every trial: "Towards the longer," or "Towards the shorter." A head-rest brought the eyes of the observer 90 cm. from the frame. A gray card, placed just before the stimulus, aided fixation and also helped to eliminate after-images. Great care was taken to rule out secondary criteria. *A*, *B* and *C* took part in the experiment.

Suggestion Group

Observer *A* gave comparatively few inversions. With the variable 4 mm. larger, he reported, with no doubt at all, "shorter." With a change of 5 mm. he said "larger, doubtful;" with 6 mm. he was sure of "longer." In another trial a decrease of 3 mm. was judged "longer" with no doubt. There were many "doubtful" and "equal" judgments. *A* declared his suspicion of the use of suggestion almost at the beginning of the experiment, and said: "I seem to be more suggestible in this experiment than in the others."

Observer *B* showed in tone of voice and in manner that he questioned the nature of the experiment; and failure in four series led *E* to discontinue it. There were no inversions, but the judgments were mostly "equal" and "doubtful."

Observer *C* gave no inversions when the change was actually towards the smaller. There were, however, many "equal" and "doubtful" judgments with changes as great as 5 and 6 mm. *C* reports that, when *E* says the change will be in one direction, "the suggestion" seems to make it just the opposite.

Control Group

Observer *A* finds the "readiness" or "agreement" that he has reported before. The typical introspection shows little or no imagery.

He says, e. g., "Attention strong on second stimulus; 'shorter' came out immediately upon clear visual and kinaesthetic perception of the stimulus."

Observer *B* gives his judgment characteristically by "yes." "A peculiar auditory after-image immediately succeeded *E*'s voice, a sort of echo accompanied by verbal idea 'yes, smaller.' I not only say to myself 'smaller' but say to myself 'yes, smaller.' Sometimes I think it quite irrelevant whether I tell you my judgment or not. The fact that I say 'yes' to myself seems to show the working of the suggestion." "Strong repetition of instructions seemed to constitute the suggested consciousness. When I actually do see the second stimulus it is surprisingly more different than I expect. That surprise is mainly kinaesthetic and organic, partially in verbal comment as if setting of mouth to say 'why?'"

Despite the verbal set, *B* reports that there is no tendency to give a judgment before the second stimulus is perceived.

Observer *C* reports a good deal of imagery which appears to be largely responsible for the 'equals' and 'doubtfuls' of the suggestion group. "The perception of the white is not constant; the apperceptive process undergoes development and changes."

Results of Experiment 4

(1) This experiment was not successful. Only in one or two cases do we find inverted judgments given with an assurance as great as (or greater than) that of the correct judgments.

(2) The types of judgment—motor, imaginal, sensory and mixed—are fairly evenly represented. *A* and *B* tend here toward the motor, *C* is definitely of the imaginal type.

Experiment 5. Noise Intensity

The point marked 45 on the graduated arc of the Fechner pendulum was chosen as that from which to give the standard stimulus. The steps used for the variable were the divisions of the scale. The observers were instructed as in the former series. The interval between standard and variable was 8 sec. The suggestion was given before every trial; "Towards the louder," or "Towards the fainter." *A, B* and *C* took part in the experiment.

Suggestion Group

Observer *A* stated near the beginning of this experiment that he gave himself the counter-suggestion of equality of the stimuli; and *E* did not venture to give many supraliminal changes. With the suggestion "Towards louder" *A* reported: 38, "Weaker, very doubtful;" 42, "weaker, doubtful, just a trifle weaker;" 41, "equal or a trifle louder; second seemed to have more volume;" 40, "equal or a trifle louder." With the suggestion "Towards fainter" *A* responded: 47, "equal, doubtful;" 49, "louder, doubtful;" 50, "trifle louder;" 51, "*weaker*, not very much weaker, but my judgment was fairly sure." A reversal of stimulus-change, made at the end of one series, was accepted without comment, as if a continuation of the series. All of these changes were supraliminal.

Observer *B* reported: "Sometimes it seems to go opposite to what you say it will. I sometimes say 'doubtful' and wait till another step." With the suggestion "Towards louder" *B* replied: 42, "doubtful, a difference, not very sure whether louder or not;" 40, "equal;" 38, "equal;" 37, "certainly sounds less;" 36, "less." *E* then gave a large supraliminal change in the opposite direction, and *B* merely said "more." accepting the trial as a continuation of the series. With the suggestion "Towards fainter" *B* answers: 51, "about equal;" 50, "equal;" another 50, "That was less; that was quite surprisingly less;" 52, "That wasn't less, equal if anything;" 53, "equal;" 55, "equal;" and in this series it is only at 56 that *B* says "louder." *E* then altered his direction, and gave 40. *B* said: "Less, not as sure about it as other, but no doubt." This change, too, was accepted as a continuation of the series; although, just before it was begun, some practice trials had been made in both directions.

Observer *C* reports suspicion of the experiment. Yet with the suggestion "Towards louder" he responds: 42, "louder, sure judgment;" 36, "very slightly louder or equal;" 41, "louder;" 39, "*louder*" (*C* adds here that he could "judge confidently; my attention has been good, hence the confident tone of voice"); 38, "louder," again with confident voice. With the suggestion "Towards fainter," *C* gives at 51, "fainter, fairly confident."

Control group

Observer *A* gives his judgments nearly always by "yes" or "plainly" or some like word. In nearly every case the analysis of judgment shows the intervention of anticipatory auditory images. The situation differs from the preceding, in that now there is no self-suggestion of the equality of the two stimuli.

Observer *B* often refers to the automatic release of the response. "The word 'yes' was almost forced upon me, as if I could not say anything else."

Observer *C* also reports several cases in which the judgment was touched off automatically. He has, however, a good deal of imagery, visual and kinaesthetic, and once mentions an anticipatory image of sound.

Results of Experiment 5

(1) All three observers give inverted judgments with as much assurance as the judgments of the control group.

(2) Reversal of direction of the stimulus-change is accepted by *A* and *B* without surprise or comment. With both observers, the changes involved were large.

(3) The judgments in this experiment were characteristically of the motor type. With *B* these motor judgments are largely preponderant; and with *A* (who inclines strongly to the sensory) and *C* (who inclines to the imaginal) they are in the majority. *A* reports the almost constant appearance of auditory images.

Experiment 6. Pitch

The standard stimulus was a tone of 870 v. d. from the Stern variator; the unit of change, up and down, was 2 v. d. The duration of the stimulus was 6 sec., increased slightly for the largest changes. The observers sat about 5 m. from the apparatus. The instructions

were: "I am going to give you a continuously changing tone, and I want you to report as soon as you notice the change in pitch. Please indicate also the direction of the change. The change in the first trial will be very small, and will be increased in each succeeding trial until you notice the change." The suggestion was repeated before each trial: "Towards the higher," or "Towards the lower." Observers *A*, *B* and *C* took part in the experiment.

Suggestion Group

Observer *A* was so suspicious of the experiment that but few series could be taken. There was, however, one inversion of judgment with supraliminal stimulation.

Observer *B* was also very suspicious, and the series were therefore curtailed. There were, however, clear supraliminal inversions: e. g., with the suggestion "Towards higher" and the stimulus-change to 860 v. d., *B* judged "higher."

Observer *C* declared, before the end of this experiment, that he thought the experiments were on suggestion. *E* told him emphatically that he was making a study of limens, but that he did not wish to explain further. The few results obtained are the more striking as *E* gave *C* on request a number of trials for practice, both up and down, at the beginning of the series. Most of the inversions occurred at 6 v. d.; but once, with the change of 8 v. d. higher, *C* replied "down" with no comment; and once, with the change of 8 v. d. lower, he replied "higher" with no comment.

Control Group

Observer *A* finds nothing new in introspection except the prevalence of tonal imagery.

Observer *B* reports characteristically by "yes" or some like word. Once he says: "My readiness to make the judgment was present before the tone came."

Observer *C* still reports much imagery.

Results of Experiment 6

(1) This experiment was not successful. A few inverted judgments, especially with observer *C*, show an assurance equal to that of the control group; but the results are very scanty.

(2) *B* gives judgments chiefly of the motor, *C* of the imaginal or mixed type. *A*, as in the preceding experiments, is of the sensory type.

Experiment 7. Noise

Series of 10 stimuli were given by the modified acoumeter; 8 with the larger lead ball commonly used; 2 (at varying places in the series) with a cotton ball made compact by a few stitches of thread and blackened with ink. Preliminary observations were taken with the lead ball, in order that this sound might, in the experiments proper, be made definitely supraliminal. The observers were seated

at a distance of 3.5 to 4.5 m., from which they could easily see the ball drop. The intervals between trials were of 8 sec. duration. The instructions were: "I want to make a few preliminary trials to find the limen roughly and see if I can make use of the experiment. Tell me whether or not you hear the sound from the ball dropping. You are to watch the ball, and to report after you see it drop." The suggestion lay in the fall of the cotton ball. Observers *A, B, C, G, I* and *J* took part in the experiment.

Suggestion Group

Observer *A* expressed surprise that he saw the ball drop but did not hear it. Once he "heard it hit the side after it dropped;" and once he replied confidently with "Yes." A little later he said he believed *E* was using two balls and trying to 'fool' him.

Observer *B* reported hearing the cotton ball three times; once he gave the judgment in a "yes" that indicated conviction. During one series he remarked: "When I don't hear it I think it is a lapse of attention; it takes about all the attention you have."

Observer *C* often reported that he heard the ball drop. His reports were given with full assurance, notwithstanding the fact that he several times failed to hear the ball and complained of lapses of attention.

Observer *G* reported 'hearing' the ball drop once; *I*, once; and *J* not at all. Failure was due, in part at least, to suspicions on the part of the observers. *G* expressed some doubt about her judgment, although she called the experience a "perception of sound;" and her final judgment was "positive."

Control Group

No introspections were taken from *G* and *J*. The two following reports from *I* are typical.

Lead ball used: "Visual perception of falling shot; fell in straight line. Auditory perception: sound of falling shot; quality clear; auditory image of ticking of clock. Duration very short; sound faint and far away."

Cotton ball used: "Visual perception of falling shot. Seemed to fall faster than before. Auditory perception, sound of shot falling, clear; duration longer than before. Sound quite loud and unmistakable; visual and auditory image of dried pea falling on glass counter."

Results of Experiment 7

We have found cases—a considerable number with one observer—in which a cotton ball is 'heard' to drop just after a lead ball has given a definitely supraliminal stimulus. The judgments are passed with assurance; and they are not corrected later, although the true stimulus is immediately repeated. There is nothing to show that the suggested experience could be distinguished from that aroused by adequate stimulus.

Experiment 8. Smell

Two paper funnels projected from a small closet in which *E* sat during this experiment. *E* often made various noises to suggest the presence of apparatus; no odors were used. The observer sat about 3 m. away, directly in front of the funnels. The instructions were: "I shall give you either a pleasant or an unpleasant odor, which will come from one of the funnels, and I want you to tell me as soon as you notice it. I shall keep the time with a stop-watch. As soon as you report the first odor, I shall give you another which will neutralize the first, and I want you to tell me as soon as the first odor has disappeared." The suggestion was made before each trial: "I shall now give you a pleasant [or unpleasant] odor." The observer was asked to give as full an introspection as possible after each successful trial. Observers *D* and *E* took part in the experiment.

Observer *D;* suggestion of "unpleasant odor." Duration, 31 sec.: "Got just one whiff of unpleasant odor; it smelled like hydrogen disulfid. I had a decidedly unpleasant odor." Duration, 106 sec.: "Before getting the smell there was a watering of the eyes, then rapidly followed a tickling in the nose and in about the same interval the smell came. I tried hard to get smell all through period; concentrated attention to get it. It does not seem to stay or grow more intense but comes and goes. The odor was very clear; I do not believe it could be any clearer. It burst out quickly; got no after-image. My eyes smart a little now." Duration, 97 sec.: "Same characteristic tightening in forehead and sensations in throat. The smell is the same I have spoken of only more intense. It was disagreeable and I smelled it a long time before it was disagreeable; not very clear at any time, but got the odor definitely; what I reported I got." Only two positive results came with the suggestion of "pleasant odor." Once *D* said: "I can't really smell it, but think there is a smell there because of the change in blood pressure;" and again: "Slight odor; no pleasantness or unpleasantness."

Observer *E:* Suggestion of "unpleasant odor."—Duration, 96 sec.: "It was rather irritating; seemed to irritate lining of nose and throat; something like ammonia. It wasn't intense enough to be really unpleasant. I could only get it by keeping my attention on it. Fairly clear." Duration, 97 sec.: "The unpleasant odor seemed fainter than before, and I was rather conscious of smelling something than of a particular smell. It was irritating. It was not clear."—Suggestion of "pleasant odor." Duration, 56 sec.: "Very faint and of a pleasant soothing nature. I got it as an odor only, that is there was no irritating effect, and I got it in the fore part of my nose and it was fairly clear." Duration, 134 sec.: "It was pleasant, of a soothing, sweet nature; I got it in the fore part of my nose. It was rather clear and lasted about three minutes."

Results of Experiment 8

(1) The reports of odors are given with full assurance.
(2) Although the reports mention various modes of tactual experience, yet they distinguish these from the experience of smell proper.

Experiment 9. Taste and Smell

Specially prepared small candies were used. Half of them were wrapped at the time of the experiment in tin foil; these were also of different shape from the unwrapped. All were made at the same time, and were of the same material and the same size. The foil-covered candies were called A, the others B. The observer tasted A (or B) for 15 sec.; then rinsed the mouth (about 30 sec.); then tested B (or A) for 15 sec.; and then gave his judgment. The instructions were: "I am going to give you two stimuli, and I want you to make a careful comparison between the two as regards gustatory and olfactory sensations." *A, B* and *C* took part in the experiment. Each observer made 10 comparisons. Differences were reported as follows:

Observer *A:* A sweeter 6; B sweeter 2; A more bitter 4; B more bitter 1; A more olfactory 4; B more olfactory 1; A smoother 2.
Observer *B:* A sweeter 1; B sweeter 6; A more bitter 4; B more bitter 1; A more sticky 6.
Observer *C:* A sweeter 2; B sweeter 3; A more bitter 4; A more olfactory 2.

The following are typical introspections. Observer *A:* "In A the sweet tasted first quite intense, then bitter, with smell quite intense. Intensive changes or alternations. A also is harder than B. In B the sweet is less intense than in A. Bitter is a long time coming as also is the smell, neither so strong as in A. No such alternation of large-small intensity as in A." Observer *B:* "A called forth first tactual and vague kinaesthetic sensations, sweet, not strong, some slight cold sensations in middle of tongue. Finally a bitter almond taste, not quite as strong as the sweet. Touch outlasted them all. B was very much sweeter, sticky, and the last burnt smell came only immediately before rinsing the mouth. The kinaesthetic sensations from chewing were somewhat clearer than in A, but not more intense." Observer *C:* "B held at the tip of tongue. At first a very bitter sensation there. Then the stimulus was broken up. Now sweet sensations for a considerable period mild, then becoming very strong with a very faint admixture of bitter sensations. A held at tip of tongue. Stronger and more widely distributed bitter sensations than with B. Then the stimulus was broken up. Now sweet sensations for a time mild, then becoming seemingly as strong as those of B. But, perhaps, not maintaining this high intensity for so long a time. Admixture of stronger bitter sensations than with A."

At the close of the experiments the observers were asked if they had noticed any constant differences between A and B. Observer *A* decided that A was generally smoother, softer and sweeter; quite often more bitter and having a stronger smell than B; taste and smell sensa-

tions rise more quickly in A than in B. (His reports show that A is generally reported as sweeter, more bitter and more olfactory than B; B also is reported as harder than A, and A as smoother than B.)

Observer *B* decided that B was sweeter and more sticky than A. (This agrees with the reports, which show that B was sweeter and more sticky than A; B is also reported, however, as more bitter.)

Observer *C* decided that A and B were the same, except that A was the more bitter. (This excess of bitter is the difference most prominent in his reports.)

Results of Experiment 9

(1) The reports of difference are given with full assurance, and differences are often reported as large, although the observers, especially *A*, had their suspicions that the stimuli were identical.

(2) The judgments are all (except in one instance with observer *C*) of the direct sensory type.

Experiment 10. Pressure

Stimuli were applied to the back of the hand by the ivory button of the Wundt apparatus; 50 gr. was the standard; change was made by steps of 2 gr. Application was for 1 sec., and the interval was 5 sec. Care was taken to prevent fatigue. Instructions were as in other experiments. The suggestion was repeated before each trial: "Towards the lighter," or "Towards the heavier." Observers *D* and *E* took part in the experiment.

Suggestion Group

D gives many "equal" judgments. With suggestion "Towards heavier," and lighter stimuli 6 (once), 12 (once), and 8 (several times), he says "heavier." Twice *E* reversed the direction of stimulus-change, to avoid suspicion: in the one case, in which the second stimulus was lighter, *D* said "heavier" at 6, and "equal" at 8 to 16; change was then made from 16 lighter to 8 heavier, and *D* accepted the stimulus as a continuation of the series, and still judged "equal."—With suggestion "Towards lighter" there were no inversions, but many judgments of "equal."

Observer *E* reported "heavier" when the stimulus was 8 lighter (twice) and 10 lighter (twice). There were no inversions with the suggestion "Towards lighter," though there were several "equal" judgments.

Control Group

Observer *D* reports a great deal of imagery, visual, verbal-auditory, and anticipatory tactual. Thus: "Sensation from first stimulus; images left consciousness entirely, tactual sensation very clear. Then tactual image came, and verbal-auditory image of word 'strong.'"

Observer *E* also describes much imagery, chiefly visual.

Results of Experiment 10

This experiment was inconclusive. Several inversions, however, occur with full assurance. Reversal of direction of stimulus-change was possible without comment from the observer. Judgments were predominantly of the imaginal type.

Practice experiments made with observer *F* and another whom we shall call *K* (a man taking advanced work in psychology) led to results that may be mentioned here. Discrete stimuli were given with a small spring pressure-apparatus, and no misleading suggestion was employed. The observers were told only that two pressures would be given, the second heavier than the first, and were asked to make as full introspections as possible.

Observer *F* reported: "First noticed the instrument before it touched my hand; then felt pressure and tried to carry it over in memory; held breath till second came. Second more intense; slight pain; seemed to feel it coming by degrees till instrument touched hand." *E* asked *F* to say more about the fore-period, before the stimulus was applied. "First feeling was less intense. It seemed to last longer than the pressure itself, quite a little longer. It was clear and definite, but much less intense than the pressure; seemed to get nearer by jerks. Just as clear as actual pressure, only less intense. Remember surprise at feeling it before stimulus touched my hand." And again: "First felt pressure near third finger; then felt touch from first stimulus, much less intense than pressures in other experiments and not lasting so long. . . . The first [pressure] before the stimulus touched the hand . . . was like a big surface; seemed heavier than the two following [from stimuli], not so clear cut; other two real ones seemed less intense than the first. . . . All three seemed about the same in clearness. The first, *i. e.*, the one before the stimulus, seemed to be of longest duration. The two from the stimuli seemed to have smaller area."

Observer *K* reported: "Auditory perception of *E*'s voice. Some imagination, imagining what feeling would be like. Really, a tactual sensation before the stimulus was applied; rather diffuse, seemed much lighter, smaller, to cover less space than perception with stimulus. The real perception much clearer; following the perception was an after-image." *K* added that the 'imagination' of sensation before the stimulus was applied "would be as clear as a very light sensation produced by the instrument."

In these reports we have a mention of anticipatory 'images' which seem closely to simulate sensation.

Experiment 11. Temperature

Cylinders 1 cm. in diameter were used; 35° C. was the standard stimulus, and the variable increased or decreased by 1° in successive trials. Instructions were as usual; the suggestion was repeated before every trial: "Towards the warmer," or "Towards the colder." The stimulus was applied for 2 sec.; the interval between stimuli was 5 sec. The inner side of the fore-arm was stimulated. Observers *A*, *B* and *C* took part in the experiment.

Suggestion Group

Observer *A* said after a few trials that he thought the stimuli of each pair were probably the same. However, with the suggestion "Towards colder" he judges 4° warmer as "colder;" 5° warmer as "equal;" and in another series, 3° warmer as "colder, doubtful." There were no supraliminal inversions when the second stimulus was colder.

Observer *B* with suggestion "Towards warmer" gives one inversion, at 2° colder. With suggestion "Towards colder" he judges 3° warmer as "colder" (twice), as "slightly colder" and as "equal"

Observer *C* with suggestion "Towards warmer" misjudges only once; 2° colder is called "warmer." With suggestion "Towards colder" he calls 3° warmer, "very decidedly colder;" 4° warmer, "very slightly colder or equal;" 6°, "equal;" 7°, "colder." After this judgment *C* said: "These judgments are more marked, the judgments more confident, than in the case of the visual sensations."

Control Group

Observer *A* several times reports anticipatory images. "*E*'s voice . . . in background; but was sufficient to arouse visual and tactual image of coming stimulus." Again: "Tactual image of how the stimulus would feel." Again: "*E*'s 'ready' and 'now' though heard inattentively caused increased strains and anticipatory tactual image." Once he speaks of a "tactual and cold(?) image."

After one of these reports, *E* asked *A* why he used the words "image" and "sensation" as he did. *A* replied: "If I had used the word 'sensation,' that would have meant that you touched me or that I thought you did. A touch sensation is different from a tactual image. I should say there is generally more of a sensation. I mean that the image is abbreviated, does not contain all that the sensation contains; perhaps, is of less extent, less intensity, generally, and I think (keeping this experiment in mind) that its extent is less clearly defined." After another experiment he remarked: "The criterion is probably the source of the stimulus, though why I answered as I did was probably unconscious."

Observer *B* reports: "Here I had a good anticipatory cold image, localized on the fore-arm before the stimulus was given. Verbal comment in imaginal terms: 'Why, here I am feeling cold.' Attitude of surprise." Asked why he used the word "image," *B* replied: "Might have been 'sensation' if the tactual sensations had also been present." In another trial *B* says: "Slight pressure and distinct warm sensations from the time *E* said 'now' up to the giving of the second stimulus." Here he uses the word "sensation."

Observer *C* reports "a diffuse cutaneous image of warmth in a certain cutaneous region of the forearm. Under the influence of the *Aufgabe* this became more intense." Again: "In mid-period kinaesthetic-auditory images 'the next will be warmer;' then attitudinal process meaning roughly 'It is really so;' then cutaneous image of warmth, warmer than first [sensation] but not so warm as second proved to be; feeling of surprise." In differentiating between "sensation" and "image" at the time of these accounts, *C* said that, in the case of the sensation, there were other intermingled sensations. There was also a different attentional accommodation; the sensation was more intense and compact; the image was, as a rule, less intense.

Results of Experiment 11

(1) The suggestion group shows inversions of judgment for all three observers; the judgments were made with full assurance, and in the face of some suspicion as to the nature of the experiments.
(2) The various types of judgment—motor, imaginal, sensory and mixed—are in this experiment fairly evenly represented.
(3) Anticipatory "images" of fair intensity play a part in the judgments.

E. Discussion and Conclusions

We have relied, in this paper, upon the reports of trained observers; and we have been obliged, in consequence, to utilize the observers *A*, *B* and *C* for experiments upon which they entered with definite suspicion. The effects of the verbal suggestions were thus, we may suppose, markedly lessened. Unless, however, we had the analyses given by these observers, we could hardly place any high degree of confidence in the positive statements of the less trained observers; types of judgment must be distinguished before interpretation is possible. It is regrettable that the limits of space have prevented our quoting in full some of the elaborate reports of the Control Group; still, enough has probably been given to indicate the general nature of the work.

Our first difficulty, then, lay in the necessity of recourse to trained observers. These men, just because they were trained, were less suggestible than the average and were likely to suspect the character of the experiments. A second difficulty arose from the necessity of prolonging the single observations. If a continuously changing color is to be wrongly 'seen,' or if a second noise is to be wrongly 'heard' in relation to a preceding noise, then the color must not change too quickly and the sounds must not occur in too quick succession. Our durations and intervals were chosen, after preliminary experiments, as times that should be long enough for the working of the suggestion and yet not so long as to preclude the possibility of a direct sensory judgment. We cannot pretend to have made them optimal; further work is desirable, with variation of times, just as further work is desirable with more trained observers and with other modes of stimulation.

Our positive results may now be summed up as follows:

(1) We have found three types of suggested consciousness, yielding three typically different kinds of judgment. (*a*) In some cases we note a carriage of the verbal suggestion, as if across the sensory contents, to the far end of the arc, i. e. to the motor expression of judgment. If it is too much to say that the sensory contents in such cases are irrelevant, we can

at least affirm that rather startling approximations to that state of affairs have occurred in the course of the experiments. The judgments of *B*, in particular, are largely of this type; they seem to represent the ripening of a motor impulse; they 'go off in a Yes' or 'say themselves.' It is hardly questionable that many instances of 'sensory suggestion' are, in reality, of this *motor* sort. (*b*) In other cases we note a free play of relevant imagery: of imagery that is largely due, in all probability, to the long-drawn-out character of the observations. The imagery clearly serves, in many instances, to furnish a new context or setting to the presented stimulus, and thus influences or determines judgment; in other words, the judgment expresses an imaginally modified situation, and does not bear directly upon the stimuli. Fully half of *C*'s judgments, and rather more than half of *E*'s, are of this *imaginal* sort. (*c*) In a third or *sensory* type of judgment, imagery is either lacking or fleeting and apparently irrelevant, while there is no indication of any motor impulse to express the suggestion. Judgments of this kind are the most frequent with the remaining observers and form (so far as we can judge) just over 40 per cent. of the total number of judgments passed in the Control Group. They alone can furnish evidence of a truly 'sensory' suggestion. (*d*) Lastly, there are judgments which cannot be placed outright under any one of these three rubrics, and that must therefore be classed as *mixed*. The combination imaginal-sensory is fairly common, and observer *A* gives a good number of judgments in which the sensory and the motor determinations are both involved.

(2) We have found sensory suggestion, vouched for by sensory judgments, with color and light (continuous change), with 'liminal' noise, with smell, taste and temperature. Our experiments with grey discs and tonal pitch (continuous change) were unsuccessful, mainly, it appears, by reason of the suspicion of the observers; those with visual extents failed, partly for the same reason and partly because the experiment favored other than sensory judgments; and those with noise intensities and pressures were inconclusive, because the experiments favored respectively motor and imaginal judgments. Other observers and other experimental arrangements might, of course, yield positive results. We have found, also, 'images' of touch, warmth and cold which simulate the corresponding sensations.

We do not hesitate to draw the conclusion that, in certain departments of sense, a verbal suggestion may arouse conscious processes which are, phenomenologically, identical with those

ordinarily aroused by an adequate stimulus or change of stimulus. The departments are those of sight, smell, taste and temperature. We think it probable that hearing and touch may be added to this list; but for those senses our evidence is incomplete or inconclusive. We have already pointed out, on the other hand, that many of the illusions ascribed to sensory suggestion by previous writers are of doubtful character. No observation may be put down as a case of sensory suggestion unless (*a*) the type of judgment is known to be sensory, and (*b*) changes of the sense-organ (adaptation, after-image, etc.) have been prevented by a fitting control of the experimental procedure.

We do not offer any physiological theory of our results, nor do we enter here upon the question of the relation of 'sensation' to 'image.' It would be premature to attempt a theory (or theories) on the basis of the present observational material; we still do not know, e. g., the qualitative range of color suggestion, or whether Baldwin's "red for green" is possible.

(3) No proof was needed that observers are 'suggestible.' We were, however, surprised at the potency of suggestion in the case of observers who had frankly avowed suspicion. *A* and *C*, the one unusually 'objective' and the other markedly 'subjective' in type, would both, again and again, 'accept' a suggestion against which they were, in general, on guard. We have here, then, a striking confirmation of a principle that of late years has won its way to recognition: the principle that the experimenter must exercise great care in the phrasing of his instructions, and must not be wearied of repeating them.

If we count the inverted judgments in the series which were taken both by men and by women, we find that the women have an excess of 3 per cent. In view of the lesser training of the women, we regard this difference as negligible.

PSYCHOANALYSIS AND THE STUDY OF CHILDREN AND YOUTH*

By Dr. O. Pfister, Pastor in Zurich
Translated by Frederick M. Smith, Clark University

By psychoanalysis is understood a method established by Prof. Sigmund Freud of Vienna, which without resorting to hypnosis investigates the unconscious contents and motive forces of the mind. That such psychical powers rule below the threshold of consciousness cannot to-day be denied. Great poets and other students of human nature have long asserted that the most exalted achievements of the human mind have sprung up out of the unconscious. Exact psychologists like Janet, James, Forel, Flournoy, nay, experimental psychologists like Ach and Offner, have demonstrated conclusively the presence of formative forces beyond consciousness. The only question is, what rôle do they play? Shall we compare them, to recall a well-known children's game, to the magnet rotating in a canister, which causes the figures placed on top to dance? Or conversely does the unconscious depend primarily upon the conscious, and is it similar only to the dragging anchor which checks the headway of the ship? Or has the unconscious the significance of land or fresh water which originally rose from the ocean as vapor and then fell from the heights, in part to nourish unseen the roots, in part to reappear in changed form as springs? No comparison is fully suitable to the facts. If I prefer to compare that unconscious with which psychoanalysis is concerned to the land or fresh water it is for these special reasons: the conscious is no more a mere puppet, as several analysts formerly believed, than are free flowing currents without effect. But the unconsciousness is not merely ballast, as others have declared, but a mighty, formative, psychic reality which influences the conscious as much as it is influenced by the conscious.

Can it really be possible to descend into the realm of this earth spirit? Psychoanalysis promises that we may do so. By what means this is accomplished can here only be sug-

*An address prepared for the International Congress on Home Education, to meet in Philadelphia in September, 1914, but adjourned on account of the European war.

gested; it subjects the phenomenon to be analyzed to sharp scrutiny and gathers all kinds of fancies which apparently make their appearance fortuitously in this connection. These associations yield a conglomerate which upon a more exact examination can be explained as a significant whole, just as one classifies portions of an exhumed skeleton as parts of an organism.

When I assert that the unconscious is the goal of psychoanalysis and that its technique is the collection of associated fancies to an apperceived structure as well as the interpretation of these fancies, then I believe I have named fully the characteristics of its concept.

Already psychoanalytical work has taken such a range that a brief review must limit itself to mere allusions. First I should like to express my sentiments concerning the objects of psychoanalysis.

1. The Objects of Psychoanalysis

At first only adults were considered whose mental or bodily derangements reverted to psychical causes. But thereby one was led to occurrences or even mere fantasies which are connected with the earliest childhood up to the age of perhaps four years. In no adult were these infantile roots lacking. Thus even the study of the adult became child study. Then gradually was awakened the courage to subject afflicted children to serious analytical treatment, as pedagogical scruples had till then stood in the way of the direct examination of healthy children. The few child analyses, those other than pedagogical and medical, brought equally favorable results (grave phenomena of anxiety were under investigation), which essentially confirmed the conclusions as to the psychical life of children which had been drawn from the examination of adults.

That we might pass from gray theory to the green meadow of direct perception let us be reminded that even analysis in the beginning turned itself to the individual symptoms alone. I first select a paradigm that may by virtue of its simplicity and lucidity gain the favor of the hearer. A boy is one morning dumb, and later in the forenoon sees everything enveloped in deep darkness and collapses in the effort to arise, while he senses a line of pressure across the breast. From a single subsequent investigation it developed that on the previous evening he had been about to confess a dereliction to the mother. Prevented in this by feelings of shame, he said to himself dejectedly: "I cannot even speak as I will! Now it

becomes completely dark about me! I hang merely by a thread!" One sees in a moment that the hysterical symptoms of dumbness, the dimness of vision, and the tension upon the breast only express symbolically the thoughts of the previous evening. They are, then, the expression of an autosuggestion, but they serve also the unacknowledged wish to escape a burdensome duty, for the dumb person can indeed make no confession, the half-blind cannot clearly see his task, the one hanging by a thread cannot firmly tread the ground. But this intention (and this is especially to be emphasized) was totally foreign to consciousness.

It may seem peculiar to the novice, yet the stubborn facts force the admission, that all psychoneuroses without exception form illustrations of such unconscious wish introduction. Impulse emotions lie at the base of them, and where an impulse neither reaches its goal nor is eliminated, a wish always comes into existence. It may be added that in this connection this word is used in a somewhat broad sense. Psychoneuroses are the picture and puzzle book that the unconscious holds before the observer. And what a gigantic picture book it is! We think of the host of cramps (e. g. hysterical screaming, writer's cramp, hysterical laughter, hysterical weeping, stomach cramps), of megrims, bowel troubles, hallucinations, imperative ideas, imperative feelings, imperative actions, convulsions, etc., we think of the tremendous effect these phenomena exert upon humanity, and we truly acquire respect for the great charade museum of the unconscious.

Now while Freud's incomparable sagacity followed the juggling of the unconscious he made a new and singular discovery. Among the smuggler-wares of the unconscious were found many of the same articles that even the healthy carry around with them for the most part neglected as worthless. Among such is the dream, which is indeed an enigmatical picture book. Among these contrabands we must also number the errors, such as striking errors in speaking, errors in writing, errors in seizing, forgetting, etc. Finally it was found that even in intentionally meaningless actions, figures, drawings, performed without purpose the unconscious ruled. What is meaningless for the psychology of the conscious proves a shining instance to the unconscious, at the same time being biologically purposive.

Had the neurologist Freud become an investigator of childhood and of normal psychology he could not have entirely turned aside from the study of the whole individual. Even the pathological symptoms, apparently lying ever so far apart,

were most closely connected and issued from conflicts in the center of personality. Was it, then, not a natural next step to investigate the effects of those unconscious processes upon the whole life's structure which psychoanalysis made clear? The problems of the study of youth were greatly enriched. A multitude of phenomena of youth that had created wonder as absolute riddles could now be understood. One understood kleptomania, pathological lies, strange imperative actions, as e. g., counting in walking, the anxious appeal to oracles, color-hearing, morbid disinclination towards special topics in school, sexual perversity, inexplicable love towards totally unfit persons, or inability to love parents or other people, fanatical devotion to foolish religious forms, pessimistic melancholy, sullen anarchy, and a thousand other facts that from the standpoint of traditional psychology had been completely inexplicable. Though many of these studies go a step past child study yet they are quite significant for the latter because in the child age the seeds are sown which later dominate the whole program of life. Out of infantile connections, of which we shall soon speak, issue the disturbing inferiorities but also incidentally the magnificent highest achievements which had previously formed the impenetrable secret of psychology.

II. The Origin of the Unconscious Powers

Insufficient as were my somewhat arbitrary suggestions concerning the sphere of endeavor in the analysis of youth, yet it is to be hoped that they created the impression that we have to deal here with important psychological problems, even if the pedagogical result were not present at all, which to the practical paidanalyst is the chief thing. Now, however, I presume upon the healthy skepticism of my hearers and their avidity for facts and hence expect the question, Can, then, the subliminal forces whose supreme influence upon life and its highest accomplishments will not be denied, be recognized with surety and clearness?

Only proper observations can decide. Whoever is satisfied with the fact that a significant number of noted learned men of various countries found the essential theses of Freud confirmed, is so at some risk. To-day one cannot well depend upon the indolent excuse that psychoanalysis cannot be learned and that its affirmations cannot be controlled by experiment. Whoever is not too indolent or too faint-hearted in the face of prevailing theories, and who understands in general how to observe himself and to form theories, can after some trials institute some very good psychoanalytical experiments. What

I propound in the following will therefore be an invitation to the hearer to study fundamentally the psychoanalytical method in order to demonstrate and to supplement the statements here made.[1]

Psychoanalysis, as well as traditional psychology, acknowledges inborn impulses, instincts, psychical dispositions. It commits to others the task of determining the origin of these psychical realities. It acknowledges also the forgotten or unobserved mental contents which compose the potential inventory of memory of the components of perception. In all events which are germane to it it concerns itself with an unconsciousness of another kind, with highly worthy ideas and endeavors that in part were conscious but through opposing ideas and endeavors are suppressed from the conscious as painful, and in part through the same antagonistic powers are repressed and prevented from becoming conscious. In both exclusions for consciousness the unconscious can be disclosed only by its effects; but this is done, it seems to me, with the same surety that I draw conclusions from the psychical happenings of other men, although I cannot disclose them directly. Should it be a matter of repression or a holding aloof from the conscious, the unconscious, which the analysis first searches out, is to the conscious mental processes a concealed combatant who from his invisible lurking place often exercises a far greater power than if he had maintained his place in the conscious.

As an example of such repression I mentioned the dumb, half-blind, and hanging boy who in the moment of sickness had no recollection of the thought suggesting those symptoms and the wish lying behind them. Striking are the examples where a violent suffering of a decade's duration is generally known to go back to such a presentation. In the light of such experiences one understands very well how in the beginnings of psychoanalysis it could be said that the psychoneurotic suffers from reminiscences that lodge in the unconscious like a sort of foreign body.

Every repression means a further checking of development, a fixation. The impulse which is repressed by a definite achievement, a concept, a feeling, or an action, does not again venture out openly. It is the history of the pike in the carp reservoir. A glass partition was placed between the pike and the carp, and as the greedy fish dashed upon the intended prey it smashed its nose most painfully. After that it avoided its neighbor, even when the invisible barrier was removed. One

[1] I refer for the rest to my book, "Die psychanalytische Methode," published by Klinkhardt, Leipzig, 1913.

also sees in this comparison that in which the task of the analyst consists: he ought to show that the partition does not exist or that it can be overcome. It is significant for the study of youth that even in the first years of life associations are formed in consequence of repression, associations which can, not only in the tenderest years, create neurosis, but very often disturb the whole future life, as a gnarl mars the growth of the plant. The mental life can never be conceived as immobile, perhaps not even in the severest sickness. The progressive development influences repression and fixation by impelling the impulse along a path which it otherwise would not adopt, where it conforms little to the interest or natural desire of the individual. In this progressive development, then, new formations are consummated whose final result we meet in artistic, religious, ethical inspirations, as also in the little errors of every day or the significant, often surprising fictions of new disease symptoms. So far as these phenomena indicate a departure from the original course of development we can consider them the product of expulsion.[2]

This expulsion may occur in the most various ways: In the formation of strange concepts chiefly of symbolical art, in the transfer of the feeling energies to entirely different spheres of life; e. g. of love to mathematics or philosophy or religion or politics. The whole "signature'" of the character can be very strongly modified, although the original nature of man always has a very important word to say about this. The criminal of lost honor, the fanatic for truth of repressed mendacity, the fanatic for purity who has to fight with unclean desires, the heresy hunter who conquered cruel desires against comrades or animals, are named as simple illustrations. That psychoanalysis can obviate such repulsions does not more closely concern us here. We see however how the repression of a single content is followed by a holding off of other psychical processes from the conscious, namely, even those remodelling ones whose result consciousness sees in the oft-mentioned pathological and inspired achievements.

Now how shall we think of the ideas and endeavors which fall to the lot of repression and its consequences? This equally knotty and noteworthy question leads us to one of the points upon which in analytical circles a heated battle has raged. There are in substance three opposing views. The first was

[2] The teachings of the church asserted rightly as the presupposition of Christian devotion the overcoming of the "natural man," only that the goal itself is not unnatural but is the expression of the "higher nature" of man.

originated by Freud, and as above mentioned it is the sexual impulse which after its repression brings about all psychoneuroses and the other evils of repression mentioned. To clearly understand this proposition the reader must accustom himself to a careful consideration of the Freudian terminology and sex theory; otherwise he may find himself in a horrible morass and will, as many incautious critics have done, accuse Freud of having lured him there. But who is able in a few minutes to present this bold, complicated, and paradoxical sexual theory, without engendering real misunderstanding? Only a few oft-mistaken traits may be mentioned: Freud, according to his express explanation, used the word *sexuality* in the enlarged sense of love generally. He added further that sexual desire in the narrow sense enters into higher functions, as sympathy, friendship, art, religion, and can thereby lose completely its original sensual character; also ambition, striving for power, and similar egotistical motives can be augmented by sexuality. Unfortunately it is furthermore not superfluous, certain opponents to the contrary notwithstanding, to lay emphasis on the fact that Freud from the first warned that repressed sexuality may take us to the lowest and perhaps dirtiest pathway to reality. For him the emphasis lay throughout upon the psychical factors of sexual life. Upon the capacity of the nearest lying sexual goal to exchange places with social values he based the possibility of a moral and higher culture. In these thoughts which form part of the support of the whole structure, I can in no way find anything that engenders scientific or ethical offense. I fully acquiesce. Yet there are, to be sure, in Freud's theses on sexual life not a few things which have rightly encountered sharp opposition. But into that I cannot here enter further than to give a cursory glance. Only one central thesis of Freud's need be referred to. The kernel of every neurosis was considered by the founder of psychoanalysis to be the repression of an incestuous desire which is directed towards the opposite-sexed parent and the jealous hate towards the like-sexed one. Every neurotic is a hindered Œdipus, he is sick because he may not espouse his mother or kill his father; every female neurotic must be considered an Electra in fantasy. That such impulses are to be met with in many neurotics must be admitted. But I am far from finding them in all. Hatred towards the father is not necessarily based on incest; it is even found in persons who have grown up motherless. But incest with the mother need not be a renewed child-wish and really often has only the sense of a regenerated fantasy: the dreamer himself might

develop it just to become another person or to form the life of another. While for Freud sexuality and love are very distinct, for Alfred Adler they mean little or nothing. It is to a high degree remarkable that one who knows how to observe so sharply and so finely certain lines of human life, especially the infantile, is in regard to the significance of sexuality and love really stricken with blindness. Adler derived all nervous phenomena and not a few character malformations from feelings of inferiority which revert to organic defects or insufficiencies. Sickness and the forms into which life is shaped follow therefore the fictive conductance lines (*fiktive Leitlinie*) to protest manfully against that inferiority, to work against the assurances (*Sicherungen*). Even the sexual fantasies serve only this fictive endeavor, which shows life in a false light. From the same source Adler derived along with neurosis, supersensitiveness, egoism, estrangement from reality, longing for power, malignancy, pride, immolating goodness, coquettish manners, cowardliness, and many other traits of character. His scholars continued the work: Fortmüller would deduce from the manly protest the whole of ethics, Kaus mendacity, Asnaourow cruelty, Wexberg anxiety, Lint in an especial case thievish inclination. So for Adler and his followers everything neurotic and the largest part of character malformations is displaced egoism; for Freud, so far as the Œdipus wish predominates, primarily unrequited love. Thereby Freud shows himself less one-sided than Adler, the former taking the ego impulse more earnestly than the latter did sexuality and love. Only Freud believed that checking the ego impulse primarily operates devastatingly when erotic reinforcements follow. To me it seems that the ego and love impulses *in concerto* are generally not to be separated from each other; every disturbance of the ego impulse influences very strongly the adjustment of love, and vice versa. Strong love precludes feelings of unreality as powerful pride forbids the highest love impulse. Only when one goes with or strengthens the other do the disturbances mentioned by Freud and Adler arise. Erotic failures affect self-feeling, reduction of self-feeling the courage of love. The impulse to repression and its after-effects can emanate from an encroachment upon the claims of the one or the other.

While according to Freud and Adler primitive impulse emotions dominate the realm below the threshold, Jung held that even highly important cultural and ethical impulsions operate in the subconscious. For Jung neurosis emanates from the inertia that establishes itself at certain development stages

and the resultant accommodation to reality, the inwardly bidden accommodation to higher cultural achievements itself resists and indeed reverts to an earlier childish relation. Inertia and indolence dispose the natural man to shrink from obvious duty; the actual conflict between inclination and duty drives him into neurosis, thereby into infantile customs, nay, into archaic functions. The dreamer, the neurotic, and especially the one suffering from dementia praecox, is atavistic. If as a result there appear sexual or incestuous fantasies, then they are (as Adler holds) only symbolically intended. In dreams, however, there is already concealed an attempt to interpret the inwardly prompted demands; in other words, to prepare an adaptation, which is by no means to say that this attempt takes the right course. Often the dream-effort at adjustment is intended only ironically. The task in any case consists in overcoming the inertia which opposes itself to the higher plane of development. Jung's hypothesis very strongly reminds one of the verses of Grillparzer:

> "But children grow apace with years,
> And every stage betrays itself
> With restlessness and crankiness.
> Indeed, oft-times an illness shows
> We are the same and yet another;
> And in one mould are both lives cast.
> And this strange law within us rules:
> Our soul expands and round itself
> A wider circle circumscribes.
> And such illness all have endured...."
> —*Judin von Toledo.*

In 1912 Freud in more restricted terms delineated the process of falling ill previously described by Jung (*Centralblatt f. Psycha., II. Jahrb., S. 298 ff.*). I must urge against Jung the objection that (1) by no means do all neuroses spring from inhibition to adaptation, and (2) indolence and inertia are an inadequate explanation of the origin of the resistance to the moral demands. As an illustration of how a neurotic development can appear, let us relate: A sixteen-year-old girl falls ill with violent headache and because of it is removed from school by the physician. As no improvement takes place, she makes complaint to me of her condition and adds that she is tormented in sleepless nights by the fear that she will become insane. The confession is introduced through the remark made in tears: "I suffer because there is no love among men!" It is easy to discover a very strong sentiment towards the brother. All other youths are dumbheads and coxcombs. Dreams disclose incest fantasies which surely are not merely

symbolically intended. Every thought of love and matrimony awakens aversion. The brother on the other hand desires that the sister address him before strangers with "*Sie*," and is furiously jealous of her. He suffers from suicidal impulses. It was easy to remove the inhibition and the headache as also the imperative ideas. It may also be added that the actual conflict is to be regarded as the next and most important cause of the repression in many cases. Freud has emphasized this as strongly as did Jung. It may be questionable to hold as Jung did that the source of neurosis lies in the tendency to resist adjustment to reality. If the thigh of a workingman is crushed by a falling block of stone one cannot well say that the imperfect adjustment of the bone is the cause of the fracture. It is condition, not cause. So when neurosis follows the death of loved persons I consider the incapacity to yield to the situation and to draw its consequences one of the conditions which made it possible for the repression to develop. Even there the early childhood is of importance so far as it can jointly determine the general features of the disease and at any rate revive again childish fantasies.

What has been said shows that one may not represent indolence or idleness as the real cause, the "*primum movens*," of repression and expulsion and the resultant defects of character before mentioned. But there is more to be added: Repressions that spring out of childhood may place an otherwise energetic man, one conscientious to the demands of duty, in a place which to another would be unimportant, viz., before an insurmountable barrier, so that the otherwise vigorous man plays a pitiable rôle. On investigation of such striking renunciation there will always be found associations which revert to childhood. Even healthy, yes all healthy people, have their idiosyncrasies which lead and force them to see and treat facts in a distorted way, and more particularly when they simulate repressed occurrences of earlier times. Even Jung admits that many neurotics are sick from youth, with whom the cause of the derangement is therefore not to be sought in an actual conflict (*Jahrb. V, 354*). If one bears this in mind he will see that, disregarding the rôle which Jung attributed to indolence (*Jahrb. V, 422, 439*), the difference between Freud and Jung is not great. It seems to me there are two groups to distinguish which are of prime importance in the study of youth: a *retention type*, with which the association comes from the past so that even a present or previous bagatelle assumes the proportions of a threatening monster, and a *repulsion type*, which in consequence of the trying present and

gloomy future throws even a normally developed man back into infantile fantasies and their automatic, possibly morbid, realization. The retention type makes out of an unpretentious present case of necessity or task a horrible affair, out of a gnat a camel. He constructs under the ban of the past a utopian present and future. Conversely, the repulsion type frightened back from the present treats the fantasies, which everyone has had and in the course of development quietly laid down, as something of prime importance, to live in and for which is the supreme task. Both types counterfeit reality and the attitude of the man to it: the retention type sees the past into the future, the repulsion type the present into the past,—both would press the present and the future into the mould of the past. The man who is *suspended between the two* projects the repressed events of the emotions belonging to the earlier time into some kind of temporal environment; the man *thrown back,* on the contrary, projects the emotions belonging to the present into the past, so that events of infantile and juvenile period, in themselves insignificant, become important and govern the point of view towards the present. The result in both cases is the same, the man becomes directed by the dreams of the past rather than by reality; that is not saying that in the aggregate personality must suffer a depreciation (cf. poets, artists, etc.). Retention and repulsion often operate conjointly.

The present differences between the various psychoanalytical tendencies do not indicate antagonistic principles, but rather important complements which in their totality constitute a healthy progress for the new movement which is so important for child study.[3] The errors will be corrected, the excesses smoothed off, but the method established by Freud and promoted by his students advances, to the welfare of science and art, to free men from fateful inward associations. I could herein only touch upon a few of the questions drawn from the huge domain of psychoanalytical activity, and, unfortunately,

[3] Even Jung's "prospective tendencies" do not deviate in principle from Freud's prophetic dreams (*ankündigen Träumen*) except that Jung extended further than did Freud the function of the tentative anticipation of the future which is to be realized in the dream. In my "*Untersuchen über die religiöse Zungenrede*" (Jahrb. f. Psychoanal. Forschungen, Bd. III., 1 u. 2 Hälfte, also published separately by Deuticke, Vienna in 1911), I showed how in particular automatisms there is a reaching back to the past but at the same time with their help there is an attempt to attain a new future. Flournoy was the first to refer to the teleological significance of hallucination ("*Automatisme teleologique antisuicide,*" Arch. de Psychologie, t. 7, p. 113-137, 1908).

could do this only in dry generalities unaccompanied by the interesting pictures of concrete experience. I must even refrain from stating the numerous laws founded upon psychoanalysis or even the fundamentally important facts of the opposition to analysis and the transfer of consciously formed contents and feelings to the analyst.

A rich banquet awaits those who value truth above tradition and who will not sleep when the swords clash in the battle for a great good cause. To be sure, even snares, manipulated by incredible means, await those who set foot upon newly discovered lands. But whoever takes up the fight cannot but accord with the beautiful sentiment of Romain Rolloud's words: "The soul that once has looked upon the Countenance of Truth and seeks to deny it, destroys himself." That significant discoveries have already been achieved is not to be denied by even the embittered opponent of psychoanalysis. Much, yes, the most, is yet to be clarified. Whoever has the inclination and talents for earnest, arduous research work is invited to collaborate.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. TITCHENER and H. P. WELD

XX. ON THE LOCALIZATION OF PURE WARMTH SENSATIONS

By F. L. DIMMICK

In the experiments which this article reports we have studied the localization of points of warmth upon the cutaneous surface. The problem had already been attacked by Rauber[1] and by Ponzo,[2] but the results were not so definite as those obtained by Henri[3] and others in investigations of the localization of pressure. Rauber employed as stimulus the radiant heat derived from a heated metal object inserted in the holes of a small piece of wood laid upon the skin; his results, which will be noted later, were too meager to allow us to judge of the success of the method; but it would appear *a priori* that the pressure aroused by the wood must have served as a distraction. Ponzo used as stimulus a small drop of warm water, which fell from a pipette through which had been drawn a fine cotton thread knotted at the lower end. Although this stimulus admittedly aroused pressure as well as warmth, the experiments were continued in the belief that the observers were able, by direction of attention, to abstract from the pressure sensation. But there is no certainty that this was the case; and it therefore seemed advisable to repeat the experiments with a stimulus that should completely avoid the arousal of pressure.

Method

We first tried, in a long series of preliminary experiments, the method advocated by Toulouse,[4] and later employed by Kiesow and Ponzo[5] and by Piéron[6] in reaction experiments; but we found it

[1] A. Rauber. Ueber den Wärmeortssinn, *Centralbl. f. d. med. Wiss.*, 1869, no. 24, 372. We have not seen this article. Its method and conclusions are reported by A. Goldscheider, *Ges. Abhand.*, i, 179; T. Thunberg, in Nagel's *Handbuch der Physiol. des Menschens*, iii, 1905, 727; and E. Hering, in Hermann's *Handbuch der Physiol.*, iii, 2, 1880, 438.

[2] M. Ponzo, Studio della localizzazione delle sensazioni termiche di caldo e di freddo, *Riv. di Psic.*, ix, 1913, 393.

[3] V. Henri, *Ueber die Raumwahrnehmungen des Tastsinnes*, 1898, 90-141.

[4] E. Toulouse, N. Vaschide, et H. Piéron, *Technique de psychologie expérimentale*, 2d ed., i, 1911, 38.

[5] F. Kiesow u. M. Ponzo, Beobachtungen über die Reaktionszeiten der Temperaturempfindungen, *Arch. f. d. ges. Psych.*, xvi, 1910, 376.

[6] H. Piéron, Recherches sur les lois de variation des temps de latence sensorielle en fonction des intensités excitatrices, *Année psychologique*, xx, 1890, 34.

impracticable. Our observers, who had had extended practice in the analysis of cutaneous sensations, reported pressure in a large number of cases.[7] We, therefore, abandoned the method and turned to radiant heat. This means of stimulation which, as we have seen, was employed by Rauber, appears also to have been used by Tanzi in reaction experiments; he describes his stimulus as the "irradiation from the tip of a flame."[8] We resorted to an electrically-heated loop of platinum wire, which was enclosed in a tube in order to lessen the lateral radiation. The apparatus was made by running a piece of no. 20 copper wire through a glass tube 2 mm. in diameter, and another piece along the outside, and then connecting the ends by means of a loop of no. 24 platinum wire 1 cm. in length. This device was enclosed in a second glass tube, 4 mm. in diameter, in such a way that the loop came within 1 mm. of the end of the larger tube. The regular alternating current of the university lighting system was used in shunt with a Nichols' frame. An intensity of 8 to 10 volts was sufficient to bring the loop to a red glow in 1 to 2 sec.

A plaster cast was made of the arm of each observer.[9] The observer, seated at a table divided by a screen, placed his right arm, volar side up, in the cast which lay to the right of the screen. An area 18 by 8 cm. was marked off into 2 mm. squares by means of a rubber stamp. Tattoo marks were made on the median line, to insure the setting of the stamp in the same place at every trial. In preliminary experiments, a number of warm spots were found by

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[7] These experiments were performed during the years 1912-13 by Mr. F. S. Kleinman under the direction of Dr. E. G. Boring. Many different sizes of pipettes were employed, but it was found impossible to obtain drops of water so small that they were not felt as pressure, even when the height of fall was not more than 2 mm. Later, the drop was applied by bringing it into contact with the skin, to which it adhered when the pipette was carefully removed. Still later, a tuft of cotton wool was substituted for the pipette. In this case, the drop was taken from boiling water and applied as quickly as possible. Again, however, about two-thirds of the applications aroused pressure as well as warmth. A fourth modification was tried: the tuft of wool and boiling water were employed as before, but now, in addition, a thin rubber membrane was stretched over the horizontal forearm; the pressure of the membrane was kept constant by hanging three weights (in all, 2 kg.) from the two edges of the membrane below the arm. The membrane was so thin that warmth was readily felt through it, and at the same time the skin was rendered less sensitive to pressure, either by adaptation or by a heightening of the limen. Even by this method, however, pressure was not entirely eliminated; it frequently happened that in a given sitting only a few pure warmths were obtained. Mr. Kleinman also attempted a modification of Rauber's method of obtaining radiant heat; the whole arm, except the region above a warm spot, was covered with a celluloid shield, and a very hot brass cylinder was brought close to the opening in the shield. The radiation, however, was not sufficient under these conditions to arouse warmth with any great regularity.

[8] E. Tanzi, *Revista sperimentale di freniatria e di medicina legale*, xvi, 1890, 396.

[9] We thought, at first, that casts would be necessary for the identification of the points in successive trials. The use of the rubber stamp made them unnecessary; but they were still a great convenience.

means of a von Frey warm-point. Those spots which gave a warmth of good intensity, and which were well removed from other warm spots, were chosen for the experiments. The area under experimentation was shaved, to prevent the arousal of pressure by touch of a hair.

The observer was required to indicate the point of stimulation on a life-sized photograph of the arm with the stamped area marked upon it. At no time during a series was he allowed to look at his arm.

The procedure was as follows. The observer was seated comfortably with his arm in the cast, and with his eyes either closed or directed on the photograph, as he preferred. The experimenter held the end of the stimulator, within two mm. of the skin, above the spot to be stimulated; gave the Ready signal; and closed the circuit. As soon as the observer felt a distinct warmth (in one to five seconds), he said 'There,' and the experimenter immediately broke the circuit. This was done in order that the stimulation might not become too intense. The observer now localized on the photograph the spot on his arm that had been stimulated. The actual process of localization differed with different observers, and will be described later.

Three observers took part in the experiments: Dr. E. G. Boring (*B*), instructor in psychology; Mr. G. J. Rich (*R*), an undergraduate in the department; and *D*, the experimenter. *B* and *R* observed in the systematic experimentation; after the regular work was completed, *R* served as experimenter with *D* as observer. This additional work was rough, and is taken into account only generally. Its main purpose was to give *D* an insight into the problem of localization from the observer's standpoint.

For the work with *B* and *R*, fifteen points upon or near the marked area were used, and ten series were worked through, making in all one hundred and fifty stimulations for each observer. With *D* only ten points were used, and only five series were taken. All of the fifteen points were stimulated in haphazard order in every series, and a rest of at least five minutes was given between series.

Besides the series with the warm stimulus, five series with a pressure stimulus were taken. A von Frey hair-aesthesiometer was employed as stimulator. The results from these series were employed for purposes of comparison and control.

Results

Method of Localization.—Henri, in his investigation of pressure localization, finds that observers fall into two groups. The one group localizes only after deliberation and verification of the point, the other localizes the point almost immediately. Henri further distinguishes the members of the two groups as those with good and those with poor visual imagery. The first difference appeared, by chance, in our two observers; the second distinction, however, did not appear. Both *B* and *R* reported in the early experiments that localization seemed to be visual; but later they declared that it was largely kinaesthetic. The difficulties and methods of localization are noted in the introspective reports. *B* remarked, at the beginning of the experiments: "Crosswise localization was almost immediate and always first. Refer to one side or the other before looking at the photograph. Considerable difficulty as to place up and down; also transversely as to how near the edge the localization occurs, particularly in the region

C-R [see Fig. 1]. Photograph not real like the arm; lower and upper portions more real. In central part the localization is hard."

R's reports at first are not full; yet they give some indication of his difficulties. "I have a feeling of uncertainty. There are no objective criteria. Localization seems to depend upon the part of the arm attended to."

At the end of the second series *B* reported: "I get a visual image of the place on the arm, and place the point in relation first to the wrist and then to the elbow." Later on, however, he decided that the "localization was rather kinaesthetic than visual."

The same thing was noted in the introspection of *R*. "Visual image of arm and place where warmth is felt. Not very definite. Use after-images." Later he reported that he had a kinaesthetic 'feel' in the left arm when he had moved the pointer to the right place on the photograph.

D, in the rough work performed by him, localized visually. He found no kinaesthesis; but this negative result may be due to the fact that his experience was only one-third as great as that of the others. He confirmed the reports of *B* and *R* as to the uncertainty and difficulty of localization.

Qualitative Observations.—Several observations of a qualitative kind appear in the reports, particularly in those of *B*. It is to be remembered that at no time during the experiment was anything allowed to come into contact with the surface of the skin under experimentation. Yet both observers often reported 'pressure.' They were asked to attempt an analysis, in the hope that some explanation might be found. *R* found nothing; but *B*, who is unusually practised in cutaneous work, reported for one stimulation as follows. "Seemed to get a pressure. It has a dull, blurry, diffuse, smooth, almost soft, mushy character; and I should say that these terms were not merely attempts to describe the indescribable, but mean actual pressure." Again he reported: "Many of the warmths seem pressury. Once I got a sort of stingy, puckery feel, which (it occurred to me) might be what makes the warmth seem pressury. There seems to be a stage when there is a very faint sensation in which I am certain neither of the location nor of the quality. Later I come to place it and then to judge its quality. On the contrary, I did get one or two warmths definite as to quality, but very indefinite as to localization until they became stronger."

It is possible that the radiant heat causes the skin to contract, and thus produces a direct stimulation of the organs of pressure. Since, however, the results do not show that the observer is more accurate in his localizations when pressure is reported, we are forced to leave the question open.

Results of Localization.—Three general statements may be made: (1) there are very noticeable tendencies in direction of localization; (2) certain areas are preferred for localization; and (3) the error is very large.

(1) Let us first consider the tendencies of direction with regard to imaginary *xy* axes; the *y* axis running up and down through the centre of the mapped area, and the *x* axis crossing at right angles in the centre . If we examine the results of *B*, we find that the localization of points at some distance above the *x* axis tends toward *P* and the *y* axis; while that of points below the *x* axis tends toward *C* and (with one exception) toward the *y* axis. But points near the

x axis, on both sides, may be localized *either* up *or* down. It follows that our 'x axis' should be not a line but a band. Moreover, since there are two points, the one just on the C side and the other on the R end of the x axis, both of which have a tendency toward P, the band should also be somewhat turned (see Fig. 1). We may say in general, then, that the tendency is *in from the edges* of the mapped area.

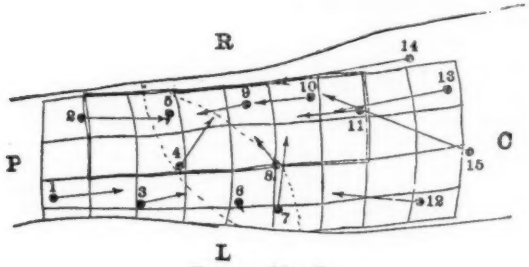

FIG. 1. Obs. *B*

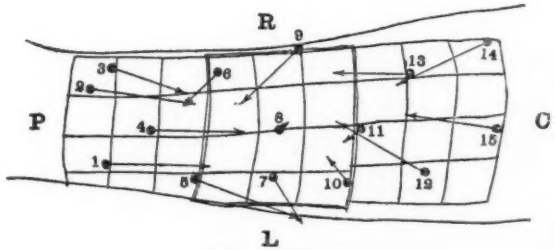

FIG. 2. Obs. *R*

The arrows in the diagrams indicate for any given point the average displacement of the localization both in amount and in direction. The values were found by computing the average component in the left-right direction and in the central-peripheral direction (adding algebraically for localizations made on different sides of the point), and constructing the hypotenuse upon these two components. The arrow-tip thus lies at the centre of gravity of all the localizations made. The length of the line does not represent the average error (Table III), which is computed independently of the direction of reference, but the average displacement in the direction indicated.

There is a striking resemblance between the results of B and those of R. If we turn to R's results, we see that the y axis coincides with that of B. The x axis is even more regular, including an area 6 cm. wide, across the middle of the mapped area, at right angles to the x axis (see Fig. 2). Here, again, the tendency is *in from the edges*.

An attempt has been made in the following Tables to give a quantitative presentation of the tendencies. Table I shows the average or resultant tendencies in the *PC* and *RL* directions (see Figg.). Both the direction of error and its amount in that direction are taken into account. If, for instance, the given localization be considered as a force, then the *P* or *C* value represents the component force in the longitudinal direction, and the *R* or *L* value the component force in the lateral direction. The resultant from these two would give the amount and direction of the point's movement.

TABLE I

THE AVERAGE OR RESULTANT TENDENCIES (IN MM.) IN THE LONGITUDINAL AND LATERAL DIRECTIONS. DOWN (P), UP (C), RIGHT (R), LEFT (L)

No. of point	Observer B				Observer R			
	Directions				Directions			
	P	C	R	L	P	C	R	L
1	....	30.5	1.9	....	....	48.2	....	4.2
2	....	38.9	....	2.9	....	46.7	....	8.7
3	....	20.2	2.5	....	....	33.6	....	13.3
4	....	17.2	20.7	....	....	43.2	....	3.5
5	0.9	....	....	3.1	....	48.8	....	13.0
6	....	0.7	....	4.7	17.0	....	....	14.7
7	....	6.2	33.6	....	....	12.2	....	22.4
8	8.7	....	13.9	....	....	3.5	2.4	....
9	28.2	....	....	2.3	28.8	....	....	28.3
10	25.8	....	....	0.4	8.5	....	11.9	....
11	28.5	....	0.7	....	8.4	....	....	5.5
12	43.6	....	6.4	....	39.2	....	24.6	....
13	52.9	....	....	3.4	35.0	....	1.4	....
14	61.4	....	....	5.4	43.9	....	....	16.7
15	66.0	....	32.7	....	40.8	....	7.8	....

In numbering the points, we begin at the *P* end of the area and go toward the *C* end. Consequently, the values in the *P* column of Table I begin at o and increase, while those in the *C* column begin high and decrease, as we go from *P* to the *C* end of the area. Those in the middle area have very small tendency-values as compared with the others.

The same thing for the lateral direction is shown in the *R* and *L* values. It does not appear so definitely, however, because the points are first on the one side and then on the other of the median line.

Since in Table I one large error in one direction may have more influence in the average than two or three smaller errors in the other direction, some of the averages do not fully evaluate the tendencies. We therefore present another Table, which exhibits the tendencies without regard to the amount of error. Table II indicates the tendencies of direction according to the actual number of localizations in every one of the four directions for every point. When either of the sums *P*+*C* or *R*+*L* is less than 10, the remaining localizations

were in a line with the point stimulated, and at right angles to the absolute direction in question; thus, if $P+C=9$, one point was localized on the line passing through the point and perpendicular to the median line. Table II, then, shows the same results as Table I, but in a different way. It also shows that the change in tendencies, as we go from the one end to the other, occurs not abruptly but gradually.

(2) The next point to be noted is the favoring of certain areas. These are outlined doubly in the Figg. For *B* the area lies along the *R* side of the mapped area, and contains 66⅔% of all localizations.

TABLE II

THE TENDENCIES OF DIRECTION ACCORDING TO THE ACTUAL NUMBER OF LOCALIZATIONS IN EACH ONE OF THE FOUR DIRECTIONS FOR EVERY POINT

No. of point	Observer B				Observer R			
	Directions				Directions			
	P	C	R	L	P	C	R	L
1	0	10	5	5	0	10	2	8
2	1	9	2	7	0	10	2	5
3	2	8	6	3	1	8	1	6
4	4	5	10	0	0	9	2	3
5	4	5	1	7	2	8	6	0
6	5	5	5	6	3	7	2	8
7	5	5	9	1	3	7	10	0
8	6	4	9	1	6	4	7	3
9	8	1	2	6	6	2	0	10
10	8	1	5	3	7	3	9	1
11	8	1	3	6	6	4	4	6
12	9	1	7	3	7	3	7	2
13	9	1	5	3	8	2	3	3
14	10	0	2	6	10	0	2	7
15	10	0	9	1	10	0	5	5

An illustration may assist in making the table clear. Spot No. 9 (Obs. B) was localized 8 times below the point, once above the point, and once neither above nor below. The same spot was also localized twice to the right, 6 times to the left, and twice neither to the right nor to the left.

For *R* the area is coincident with *x*, *i. e.*, it is 6 cm. wide, and extends across the middle (up and down) of the mapped area. It contains 50% of all the localizations. *B* reported that he was "prejudiced by the stamped area, in that there was a tendency to localize well within it." This prejudice does not, however, account for the extreme favoring of a single area, a fact for which there is no explanation in the observers' reports.

(3) Finally, the error of localization is itself exceedingly large. Of the 300 localizations made by the two observers, only 4 were correct, *i. e.*, fell within the area of 4 mm. A larger number might have been expected to occur by chance. Only 20 localizations were made

with an error of less than 10 mm. The extremes were 146 mm. for *B*, and 132 mm. for *R*. Table III gives the average errors for each point, and their mean variation.

We have here a rough measure of the inability of the observers accurately to localize pure warmth. We find average errors ranging from 15 to 63 mm., and mean variations often more than half the value of the average error.

Pressure.—It was said above that five series were performed with a pressure stimulus for the sake of comparison with the main series. The results obtained with pressure were similar to those got by Henri under under similar conditions and by the same method. The errors were comparatively small, the average error for *B* being about

TABLE III

AVERAGE ERROR (IN MM.) AND MEAN VARIATION FOR EACH POINT

No. of point	Observer B		Observer R	
	A.E.	M.V.	A.E.	M.V.
1	30.9	15.28	57.6	25.12
2	28.1	14.80	50.4	31.80
3	27.9	9.70	43.4	20.04
4	35.2	14.58	43.2	21.80
5	15.8	8.96	58.8	27.60
6	24.6	15.60	33.5	16.10
7	44.8	12.60	31.2	11.90
8	22.2	5.04	25.0	15.20
9	31.1	11.48	46.6	18.50
10	24.4	16.00	25.3	12.22
11	32.1	18.12	22.2	6.60
12	52.5	22.40	57.9	25.12
13	51.6	9.58	51.1	16.68
14	59.9	25.00	46.4	17.78
15	63.8	25.04	45.1	12.10

11 mm., and for *R* about 20 mm.,[10] and the localizations were clustered around the point of stimulation instead of tending in a single direction as was the case with warmth. It seems, then, that the observer has far less ability to determine the position of a point on the skin when it is stimulated by warmth than when it is stimulated by pressure.

Previous Work.—A comparison of these results with those of previous investigators of the localization of warmth shows both agreement and disagreement. Rauber concluded that the 'warm circles' are larger than the corresponding 'touch circles.' In so far as this statement means that the error of localization of warmth is greater than that of the localization of pressure, we agree. Ponzo reached the same general conclusion, although his average errors and mean variations are much smaller than ours. Furthermore, Ponzo's results show

[10] These average errors are larger than they should be, because only five series were taken, and a few large accidental errors raised the average.

errors of direction which group about the point of stimulation, while the results of our experiments show a tendency to localize certain points in certain areas. These differences are doubtless attributable to differences in method. Ponzo employed a stimulus that was not free from pressure, his observers localized by touching the arm, and the warm spots were localized but five times as against our ten.

XXI. Form *vs.* Intensity as a Determinant of Attention

By L. G. Meads

President Sanford once asked the senior editor of these studies if it might not be possible to compare form and intensity of stimulus, in some quantitative way, as regards their power to compel the attention. The present experiments give a first, rough-and-ready answer to this question in a single case. A light-form of varying intensity was compared with a 'formless' spot of light of fixed intensity, and that ratio of the objective intensities was determined at which the two stimuli were judged equally clear.

Apparatus.—The apparatus consisted of a projection lantern with tachistoscopic attachment; a black box with three openings, one to admit the light of the lantern, and two front circular openings (8 cm. in diameter, 18 cm. apart) covered with ground glass, through which the light passed to a large ground glass screen (150 by 105 cm.); and an episcotister placed between box and screen. The tachistoscope gave an exposure of a tenth of a second (110 ± 8 sigma). One-half of the screen was covered with a sheet of thick paper, in the centre of which was cut a square cross (9 by 9 cm.; width of arms, 4 cm.); the episcotister was placed always on the same side as the cross. A small electric bulb, set midway between the stimuli, served as fixation-point. The 'formless' spot was, of course, roughly circular; it had a bright central area of some 12 cm. in diameter, and a total diameter of some 17 cm., and was therefore considerably larger than the cross; it made an impression of vagueness and diffuseness, and the observers never named its form, but referred to it as the 'spot' or 'patch' of light.

The observations were made in the dark room and with dark adaptation. The observers sat at a distance of 3.75 m. from the screen.

Method.—The instructions were as follows: "At the signal Now! you will fixate the spot on the glass; a second and a half later, two spots of light will be shown. You are to report which of the two (if either) is the more clear." The observer was further instructed that observations would be made in series, in some of which the one and in others the other spot would at first be definitely the clearer.

In the descending series the light of the cross was cut down to 180° of the episcotister, while the formless spot was presented at full intensity; in the ascending series the light of the cross was cut down to 84°. Preliminary experiments had shown that at 180° the cross would always be the clearer, and at 84° would always be the less clear stimulus. The steps in both series were 12°.

Two observers made 40 series; ten ascending and ten descending with the cross on the right, and the same number with the cross on

the left. These observers were Professor H. P. Weld, and Mr. J. S. Smith, a graduate student. Three observers made 8 series; two ascending and two descending with the cross on the right, and the same number with the cross on the left. These observers were Mr. H. G. Bishop, assistant in psychology, and Drs. E. G. Boring and W. S. Foster, instructors.

Judgments were passed in the form 'spot' (or 'patch'), 'cross,' 'equal.' Preliminary experiments showed both that there is a tendency to name the clearer stimulus, and also that, if the judgments 'clearer,' 'less clear' are required, the attention is likely to be focussed upon the one or the other stimulus with a view to comparison.

Results.—With full intensity of the formless spot, judgments of equal clearness were obtained for the following intensities of the cross:

Observer	Degrees	M. V.
H. P. W.	135	9.2
J. S. S.	136.5	13.7
H. G. B.	132	21.0
E. G. B.	118.5	8.0
W. S. F.	142.5	16.8

The following excerpts from the introspective reports may be quoted:

E. G. B.—"I am conscious of a difference of intensity even when judging clearness in the reverse order." "The judgment of clearness seems very much tied up with intensity. I would sometimes think 'That is intense enough to make them equal in clearness,' although the intensities were obviously unequal." "I noticed the clear-cutness of the cross even though the spot was the more clear."

H. G. B.—"Easy to judge. Clearness stood out in one even though intensity was very much less in that one."

W. S. F.—"You seem to have to let yourself be blank, or rather to attend to both,—no previous attending to one. You attend to both, only one is clearer than the other."

H. P. W.—"The judgment is very easy provided only the observer can avoid any sort of expectancy before the stimulus appears. If, however, one should expect 'spot' (let us say) to be more clear, then one is very apt to judge it so, unless the difference in intensity is very great."

Conclusion.—We conclude that a light-form of relatively low intensity may have as great a power to attract attention as a formless light of relatively high intensity. In the concrete, a cross illuminated by some 133° of the light of a 25 w. 110 v. mazda lamp sent through two ground glasses may be as clear as a formless patch illuminated by the full 360°.

This is a single result, obtained with a somewhat rude technique; but the technique can be improved; and the ease of judgment renders it probable that the comparison of form and intensity can be made with other material.

XXII. The Determination of the Limens of Single and Dual Impression by the Method of Constant Stimuli

By E. J. Gates

In their discussions of the method of Constant Stimuli both Müller [1] and Titchener [2] employ a set of results which were obtained by Riecker in a determination of the two-point limen.[3] These results are far from satisfactory; but, although Titchener advocates the use of the method of Constant Stimuli for the determination of the two-point limen, nothing better has as yet appeared. It was our original purpose to repeat Riecker's experiment, not only in the hope of obtaining a more nearly regular set of results, but also as a further test of the method. Later, however, a modification of the experiment, by the admission of a larger number of categories than the usual one-point, two-point and doubtful judgments, seemed advisable. In preliminary experiments, our observers reported that, between the impression of a single point, and that of two discrete points, there occurred a number of impressions which proved to be similar to those which, since the experiments of Henri and Tawney,[4] Tawney,[5] and Judd,[6] are known as 'point,' 'circle,' 'line,' 'dumb-bell,' etc. The method of Right and Wrong Cases regarded these impressions as belonging to the category 'one-point'; but there seemed to be no reason why the judgments 'circle,' 'line,' etc., might not be accepted, and their frequencies treated as psychometric functions, precisely like the 'two-point' judgments. When our experimental work was completed, we submitted the data to Professor Urban, who assured us that, in his opinion, any number of categories might be accepted, and that the notion of psychometric functions was adequate to the treatment of our data.[7]

The Experimental Procedure. Since we set out to repeat Riecker's experiment, we worked on the lower eyelid of the right eye, and we used the same nine *D*'s: 0, 0.5, 1, 1.5, 2, 3, 4, 5, and 6 Paris lines (1 Paris line = 2.27 mm.). We employed the Griesbach aesthesiometer, with a pressure of 10 grams on each point as indicated by the scale of the instrument. We added, however, points of hard rubber, whose weight brought the pressure up to 19.5 gr.; this pressure, as measured by a chemical balance, was fairly constant; and in any case the m. v. of the instrument was probably exceeded by iregularities of applica-

[1] G. E. Müller, *Gesichtspunkte u. Tatsachen d. psychophysischen Methodik*, 1904, 38ff.

[2] E. B. Titchener, *Experimental Psychology*, 1905, II, i, 92ff.; ii, 250.

[3] A. Riecker, Versuche über den Raumsinn der Kopfhaut, *Zeit. f. Biologie*, 1874, 177ff.

[4] V. Henri and G. Tawney, Ueber die Trugwahrnehmung zweier Punkte bei der Berührung eines Punktes der Haut, *Philos. Studien*, xi, 1895, 394ff.

[5] G. Tawney, The Perception of Two Points not the Space-threshold, *Psychological Review*, ii, 1895, 585ff.

[6] C. H. Judd, Ueber Wahrnehmungen im Gebiete des Tastsinnes, *Philos. Studien*, xii, 1896, 409ff.

[7] We are glad to acknowledge our indebtedness to Professor Urban not only for his criticism and advice, which have been most helpful, but also for his interest in our results.

tion, although very great care was taken to handle the aesthesiometer in the same way throughout the experiments. The added points were very slightly conical, and had a terminal diameter of 1.15 mm. The nine *D*'s were arranged by hazard into 100 series, so that every stimulus was presented 100 times, and 900 experiments were performed upon every observer. Those who took part in the experiment were Dr. E. G. Boring (B), then assistant in Psychology; Dr. C. A. Ruckmich (R), then instructor in Psychology; and Asst. Prof. H. P. Weld (W). The observers lay supine on a low couch, with eyes closed; the experimenter sat on a chair at the head of the couch, and, holding the instrument in both hands, gave the ready signal and applied the stimulus. The judgment was given as nearly immediately as might be; and the experimenter, holding the aesthesiometer in his left hand, entered the judgment by a symbol on the record-sheet, and then set the instrument for the next experiment. The whole procedure, from experiment to experiment, occupied 10 or 12 seconds. At the end of the series, however, a longer period (30 to 60 sec.) was allowed, in order to prevent adaptation; and on the resumption of the experiment the beginning of a new series was announced. A few weeks previously, the experimenter had put each one of the observers through a practice-series of 900 experiments, so that the work proceeded with almost machine-like regularity. The experiments were performed between 2.30 and 4 o'clock on three afternoons of the week; a single sitting lasted for about thirty minutes; and in that time about ten series were run off. After the preliminary work was finished, therefore, about four weeks were required for the collection of the data.

In the preliminary experiments the observers were limited to the three usual categories. When it was decided to accept other types of judgment, the observers themselves were allowed to choose their terms. The observations of W and of R are hereafter discussed under the headings 'point,' 'point or circle,' 'circle,' 'circle or line,' 'line,' 'line or dumb-bell,' 'dumb-bell,' 'dumb-bell or two-points,' and 'two-points.'[8] Observer B adopted only five categories: 'one-point,' 'one-point or extended,' 'extended,' 'extended or two-points,' and 'two-points.' For all observers, the 'or' judgments were regarded as doubtful judgments.

The Results. The results of such an experiment consist, of course, in the numbers expressing the relative frequencies with which the different *D*'s were judged under the different categories. These results are shown in Tables I and II. Before we proceed to their

[8] These names were, in fact, used only by W. Introspectively, however, R's perceptions so closely resembled those of W, that for purposes of comparison the one set of names may be employed. The principal differences are that for W the third category included impressions which were circular, as if the stimulus was a large blunt point, while for R impressions in this category were slightly oval and nucleated; again, for W the 'line' had a definite width and was rounded at the ends, while R described it as a long narrow oval with two nuclei.

TABLE I

OBSERVED FREQUENCIES OF JUDGMENTS OF THE NINE D'S UNDER NINE CATEGORIES

Observer W

D	point	point circle	circle	circle line	line	line d-bell	d-bell	d-bell two	two point
0	81	4	10	..	3	..	2	..	..
0.5	18	2	65	..	13	..	2	..	..
1	13	3	58	8	17	..	..	..	1
1.5	14	2	59	1	14	4	2	2	2
2	11	1	56	5	20	..	3	1	3
3	3	1	29	2	35	7	16	3	4
4	..	..	7	2	24	4	19	13	31
5	..	..	..	..	1	2	3	6	88
6	..	..	..	..	..	..	..	..	100

Observer R

D	point	point circle	circle	circle line	line	line d-bell	d-bell	d-bell two	two point
0	53	12	27	1	5	..	..	1	1
0.5	10	7	50	6	17	..	8	..	2
1	4	5	52	8	18	3	8	..	2
1.5	4	11	46	9	16	2	7	..	5
2	9	9	39	4	22	1	10	1	5
3	7	2	22	4	17	1	28	2	17
4	1	..	4	..	6	4	39	1	45
5	..	..	3	..	2	..	15	2	78
6	..	..	..	..	..	2	6	3	89

TABLE II

OBSERVED FREQUENCIES OF JUDGMENTS OF THE NINE D'S UNDER FIVE CATEGORIES

Observer B

D	point	point extended	extended	extended-two-point	two-point
0	89	1	9	..	1
0.5	63	2	33	1	1
1	56	6	36	1	1
1.5	57	3	36	1	3
2	51	2	41	2	4
3	28	4	50	5	13
4	5	2	37	16	40
5	1	..	5	5	89
6	..	..	..	2	98

mathematical treatment, we may call attention to certain facts which the Tables reveal. (1) The relative number of 'or' judgments for all observers is very small. Two hypotheses suggest themselves. There may have been an unconscious tendency to avoid doubtful judgments, due either to the positive tendency to judge under a specific category, or to the mere fact of a considerable number of possibilities of judgment. Or, again, the perceptual experiences may not have 'shaded off' one into another; the regions of particular judgments may have been too large, and the categories too flexible, to permit of other

judgments. This supposition seems to be borne out by another result of Table I. (2) The total number of 'circle' judgments is out of all proportion to the others. At first thought one might suspect a change in standard of judgment; but that is unlikely, since the proportion of these judgments to the total number of judgments is approximately the same for both observers (31% for W, and 29% for R). Apparently, then, either the region was large, or the category admitted a large number of different impressions. Finally, (3) it may be observed that while the curves of frequencies are far from ideal they are yet fairly regular. Observer B has a single inversion of 1%; W has one inversion of 4% and four of 3% or less; R has one of 6%, two of 5%, and five of 3% or less.

The method of treating results where more than three categories are admitted has been indicated by Urban in his examination of Keller's acoumetrical experiments.[9] In these experiments five kinds of judgments were allowed; and since, in our experiments, B also had five categories, the method of treatment in the two cases is so far identical. The procedure, in brief, is as follows. We first determine in the usual way the constants for the psychometric functions of the One-point judgments, and for those of the Two-point judgments. Their curves obviously give the boundary, on the one hand between the One-point and the Point-extended judgments, and on the other hand between the Two-point and the Two-point-extended judgments. If, now, we combine the frequencies of the One-point and of the

TABLE III

Boundary	h′	c′	h	c	L	Interval
			Observer B			
1–1X.....	—.1963	0.5350	—.3926	—0.6428	1.636	
1X–X....	.2220	—0.6211	.4440	0.7110	1.601	—0.035
X–X2....	.2781	0.4040	.5562	2.0726	3.724	2.123
X2–2.....	.2867	0.5639	.5734	2.2841	3.980	0.256
			Observer R			
p–pc.....	—.1642	1.3180	—.3284	0.3328	—1.012	
pc–c.....	—.1712	1.3900	—.3424	0.3628	—1.060	—0.048
c–c1.....	—.1989	0.4601	—.3978	—0.7333	1.843	2.903
c1–1	—.2117	0.3891	—.4234	—0.8811	2.081	0.238
1–1db....	.2455	—0.0837	.4910	1.3893	2.830	0.749
1db–db ..	.2440	—0.0379	.4880	1.4261	2.923	0.093
db–db2...	.2328	0.4713	.4656	1.8681	4.012	1.009
db2–2....	.2333	0.5225	.4666	1.9323	4.150	0.138
			Observer W			
p–pc.....	—.2826	1.5970	—.5652	—0.0986	0.174	
pc–c.....	—.2909.	1.5880	.5818	—0.1574	0.270	0.096
c–c1.....	—.2570	0.4266	.5140	—1.1154	2.170	1.900
c1–1	.2396	—0.2553	.4792	1.1823	2.467	0.297
1–1db....	.2682	0.2379	.5364	1.8471	3.438	0.971
1db–db ..	.2735	0.3102	.5470	1.9512	3.567	0.129
db–db2...	.3154	0.6030	.6308	2.4950	3.960	0.393
db2–2....	.3316	0.7029	.6632	2.6927	4.060	0.100

[9] F. M. Urban, Ueber die Methode der mehrfachen Fälle, *Arch. f. d. ges. Psychologie*, xvii, 1910, 380-387.

Point-extended judgments, and determine their constants, we shall obtain the lower boundary of the Extended judgments; and again, if we combine the psychometric functions of the Two-point judgments and those of the Two-point-extended judgments, and determine their constants, we shall obtain the upper boundary of the Extended judgments.

The same process of calculation applies when more than five judgments are accepted. We successively combine the frequencies from the two extremes, until we arrive at three or two judgments. Where five categories are employed, as by B, we arrive at four boundaries; where there are nine kinds of judgment, as in the cases of R and W, there are eight boundaries, since the lowermost and the uppermost boundary in every case is infinity. The constants of the psychometric functions which give the curves of these boundaries are shown in columns 3 and 4 of Table III. The values of h′ and c′ are derived from the normal equations of the method of least squares. The quantities necessary for setting up these equations were obtained from Urban's tables.[10] The values of h and c are derived from h′ and c′; $h = h'/d$, where $d = 0.5$, and $c = 3h + c'$. The limens, which are derived from h and c, $L = c/h$, are presented in the fifth column of the table. In the sixth column are to be found the intervals of uncertainty. The interval represents in any case the difference between the upper and the lower boundary; for us it represents the area of the region covered by a category.

An inspection of the table reveals inversions of the first two categories by B and R. B's inversions, however, are very slight, in the second decimal; those of R are negative. The first of these probably means that even one point is judged as 'point or circle' more often than 'point.' The sense of the second seems to be that the tendency to give 'point' judgments for the least separation (0.5 Paris line) is equal to (or slightly greater than) the tendency to judge 'point' at no separation. There is good agreement as regards the Two-point limen: the average for all three observers is $4.06 \pm .08$. Riecker's Two-point limen was 1.88, a value which for our observers is approximately the limen of the first lower boundary of the third category. Riecker, apparently, either ran foul of the stimulus error, or purposely included the intra-cutaneous impressions in his Two-point judgments. The intervals of uncertainty are what we should expect from an inspection of the table of frequencies. Although it is here that the experiment is seen at its worst, we cannot say whether the irregularities are due to some inherent irregularity in the perceptual patterns, or whether they result from a change in standard of judgment. The observers reported a difficulty in maintaining the same standard and regretted that they had not had extended practice-series with the new categories. It is remarkable, on the other hand, that the interval of 'circle' is the largest interval for both R and W; that the size of this interval closely resembles that of B's 'extended;' that for W the intervals of circle, line and dumb-bell become progressively smaller as the two-point limen is approached; and that a similar relation is found between W's 'or' judgments. Aside from the fact that R's intervals do not show these relations so regularly, they differ materially from those of W only in the size of the interval for 'dumb-bell.'

In conclusion we may say that, if we were repeating the experiment,

[10] F. M. Urban, Hilfstabellen f. d. Konstanzmethode, *Arch. f. d. ges. Psychol.*, xxiv, 1912, 236ff.

we should give our observers the benefit of greater practice in judgment under the various accepted categories. We believe that a better set of *D*'s could be found; and we are sure that the cutaneous area chosen is not the best possible. We should take introspections at the close of each day's work.

Appendix

The following figures were obtained by a pair of students in the regular work of the laboratory course in Quantitative Psychology. The *E* was G. J. Rich, the *O* was F. L. Dimmick. The method of procedure, the *D*'s, the cutaneous area, etc., were the same as in our experiment. Five categories were admitted: point, circle, oval, long-oval, two-point. Fifty series were made. In the calculation, the five categories were reduced to three in the way described above, and the directions in Titchener's Manual were then followed. The quantities for the equations were taken from Urban's tables. The results are:

Boundary	h′	c′	h	c	L
pt–circle.....	.1839	—0.9092	.3678	0.1942	0.528
c–oval.....	.2158	—0.2471	.4316	1.0577	2.451
o–lo.....	.2353	—0.2903	.4706	1.1215	2.383
lo–2.....	.2913	1.2110	.5826	2.8688	4.924

The figures are a sample of the sort of results that may be obtained by conscientious work in a limited period of time (4 afternoons), by students who have had only the practice that the laboratory drill-course affords.

BOOK NOTES

Dynamic evolution; a study of the causes of evolution and degeneracy. By CASPAR L. REDFIELD. New York, G. P. Putnam's Sons, 1914. 210 p.

The author starts out with the rather startling proposition that scientists think they believe in evolution but really believe in special creation. They have made it probable that animals today are the genetic descendants of different animals of earlier ages, but their explanation of how existing animals took their present form involves nothing but special creation. Present animals are descended from uni-cellular organisms but human intelligence was not derived from man's ancestors because they did not have it, and so it must have been created. The chapters are on energy in general and in animals, horse-breeding, 2.10 trotters, the effects of two generations, English setters, Holstein-Frisians, man and longevity.

Recherches sur les sociétés d'enfants. Par J. VARENDONCK. (Institut Solvay, Notes et Mémoires, Fascicule 12). Bruxelles, Misch & Thron, 1914. 93 p.

This study is in some respects a model of its kind. It is one of the most comprehensive studies of children's societies and these are classified and the origin of the groups is described. A special chapter is devoted to the chief or leader, and the psychology of the members, the collective conscience, attitude toward strangers, are also treated.

What men live by. By RICHARD C. CABOT. Boston, Houghton Mifflin, 1914. 341 p.

These essays are of extremely different values but all are breezy and original and the whole thirty-four of them are divided into (Part 1) work, (Part 2) play, (Part 3) love, (Part 4) worship. The author's characterization of love is by far the best part of the book and is as sane and wholesome reading as we have seen for many a day and involves the highest conceptions of wedded life.

Poetik. Von RICHARD MÜLLER-FREIENFELS. Leipzig, B. G. Teubner, 1914. 98 p.

Vorträge zur Einführung in die Psychologie. Von HANS KLEINPETER. Leipzig, J. A. Barth, 1914. 435 p.

Grundzüge der Psychotechnik. Von HUGO MÜNSTERBERG. Leipzig, J. A. Barth, 1914. 767 p.

An introduction to general psychology. By ROBERT MORRIS OGDEN. New York, Longmans, Green, 1914. 270 p.

Grundriss der Psychologie für Juristen. Von OTTO LIPMANN. 2d ed. rev. and enl. Leipzig, J. A. Barth, 1914. 95 p.

Outline of a study of the self. By ROBERT M. YERKES and DANIEL W. LARUE. Revised from the edition printed for the authors in 1913. Cambridge, Harvard University Press, 1914.

Behavior; an introduction to comparative psychology. By JOHN B. WATSON. New York, Henry Holt & Co., 1914. 439 p.

An experimental and introspective study of the human learning process in the maze. By FLEMING ALLEN CLAY PERRIN. (Psychological Monographs, Vol. XVI, No. 4, July, 1914). Princeton, N. J., Psychological Review Co. 97 p.

Psychological experiments. By J. V. BREITWIESER. Colorado Springs, Apex Book Co., 1914. 187 p.

Laboratory manual for human physiology; experimental studies in hygiene, sanitation and physiology. By CARL HARTMAN. Yonkers-on-Hudson, World Book Co., 1914. 144 p.

Is conscience an emotion? Three lectures on recent ethical theories. By HASTINGS RASHDALL. Boston, Houghton Mifflin Co., 1914. 200 p.

Contributions to the analysis of the sensations. By ERNEST MACH. Translated by C. M. Williams. Chicago, Open Court Publishing Co., 1910. 208 p.

Der Traum ein assoziativer Kurzschluss. Von Dr. HANS HENNING. Wiesbaden, J. F. Bergmann, 1914. 66 p.

Report from the Department of Pathology and the Department of Clinical Psychiatry, Central Indiana Hospital for the Insane. 1911-1912 and 1912-1913. Vol. V. Indianapolis, Wm. B. Burford, 1914. 380 p.

Plato: Ion, with introduction and notes. By J. M. MACGREGOR. Cambridge, University Press, 1912. 45 p.

What is paranoia? By E. STANLEY ABBOTT. (Reprinted from American Journal of Insanity, Vol. LXXI, No. 1, July, 1914, p. 29-40.)

Anatomy and physiology of the eye and its appendages. By JOHN WELSH CROSKEY. Philadelphia, Smith-Edwards Co., 129 North 12th St. 18 p.

Certain further factors in the physiology of euphoria. By GEORGE V. N. DEARBORN. (Reprinted from the Psychological Review, Vol. XXI, No. 3, May, 1914, pp. 166-188.)

Our sexual birthright. By GEORGE V. N. DEARBORN. (Reprinted from Mind and Body, September 1914, Vol. 21, no. 231, p. 215-223.)

The self and the ego. By KNIGHT DUNLAP. (Reprinted from the Psychological Review, Vol. 21, No. 1, January, 1914. pp. 62-69.)

Images and ideas. By KNIGHT DUNLAP. (Reprinted from the Johns Hopkins University Circular, March, 1914.) Baltimore, Johns Hopkins Press, 1914. pp. 25-41.

Theodor Lipps

The death is announced, at the age of sixty-three years, of Professor Lipps of Munich, well known to psychologists by his systematic work *Grundtatsachen des Seelenlebens* (1883; reprinted 1912) and by his contributions to the theory of space-perception and to psychological aesthetics. Lipps, who has been in retirement for some years on account of ill-health, was a keen and incisive thinker, and master of a remarkably trenchant style. He was by temperament logician rather than psychologist; but a strenuous logic is indispensable to psychological theory; and Lipps' criticisms were always of positive value, though his constructive work—ingenious and suggestive as it undoubtedly was—did not find general acceptance.

Lipps' other works are: *Psychologische Studien* (1885, 1905); *Der Streit über die Tragödie* (1890); *Aesthetische Faktoren der Raumanschauung* (1891); *Grundzüge der Logik* (1893, 1912); *Zur Psychologie der Suggestion* (1897); *Raumaesthetik und geometrisch-optische Täuschungen* (1897); *Komik und Humor, eine psychologisch-aesthetische Untersuchung* (1898); *Ethische Grundfragen* (1889, 1912); *Leitfaden der Psychologie* (1903, 1909); *Selbstbewusstsein, Empfindung und Gefühl* (1901); *Vom Fühlen, Wollen und Denken* (1902, 1912); *Aesthetik* (1903-6); *Naturwissenschaft und Weltanschauung* (1906); *Philosophie und Wirklichkeit* (1907); *Naturphilosophie* (1907); *Psychologische Untersuchungen* (editor, 1907 ff.); *Beiträge zur Aesthetik* (co-editor, 1890 ff.).